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THE PSYCHOLOGICAL REVIEW

THE FORMAL CRITERIA OF A SYSTEMATIC PSYCHOLOGY

BY JOHN A. McGEOCH
University of Missouri

INTRODUCTION

A concern for system making, paradoxically enough, is a major attribute of contemporary psychology. By explicit avowal most psychologists are experimentalists and their official attitude toward system-making is apt to be one of scorn. But systems continue to be constructed and debated and even those who scoff may be found among the debaters. Behaviorism and *Gestalt-psychologie* have been, for example, far more widely discussed at the level of general theory than at that of experimental fact. The interest aroused by the systematic positions set forth in the *Psychologies of 1925* and in the *Psychologies of 1930* has been both widespread and very great; and if further evidence of the contemporary vitality of systematic problems be desired, one has only to cite the large number of general textbooks, published within recent time, which aim at the presentation of the subject from a particular systematic position and in which the facts, though important, are secondary to the interpretive framework in which they are given their place.

To some critics the term 'system' implies a closed, finished, somewhat dogmatic body of doctrine and the present vitality of system-making is to them a regrettable fact. A system must be 'closed' temporarily, it is true, though only in the sense that it aims at a unified structure, but it need be

neither finished nor dogmatic, and it is certainly not doctrine in any sense save that of tentatively held and cautiously expounded general theory. One runs the risk, doubtless, when one attempts to systematize, of lapsing into a fixed belief in the final truth of one's system and thence into the rôle of its prophet. But the risk is necessary and the prophetic rôle not unavoidable.

It has been urged, moreover, that psychology is too young and too much in need of facts to permit fruitful systematization. The important, though incomplete, systems of the last seventy-five years, many of which were essayed when the facts possessed were but a fraction of ours, present one effective answer. To this may be added a second, that new and fruitful research usually waits on suggestive and far-reaching theorizing. The facts one has at any given time are, moreover, of uncertain value without interpretation in terms of a more inclusive and organized body of knowledge. Instead of being futile, the interest in systematizing is a recognition that experimentation requires, both for its guidance and for the interpretation of its results, a wider relational structure than the laboratory alone provides. Obviously, it seems, neither fact nor system is, by itself, significant and attempts by proponents of one to decry the other must disregard the essential characteristics of a body of scientific knowledge.

The Meaning of System

By the term 'psychological system' is implied a coherent and inclusive, yet flexible, organization and interpretation of the facts and special theories of the subject. Organization alone does not constitute system in this sense, unless organization be taken to imply interpretation, although the adjective 'systematic' is often applied to an experiment, for example, which aims to work out the varied conditions of a phenomenon in an organized and relatively complete manner. For system, interpretation and interrelation must be added to the organization, not only of facts and theories in special fields, but in all the domains of the subject, in such a way as to render all of them coherent parts of a unitary structure. To this

extent, system passes beyond the facts and partial categories, judges them critically, relates them to each other.

Both in the critical examination of psychological systems and in an attempt to systematize the rapidly growing body of experimental fact, one is at once forced to inquire for what general problems an adequate system must offer a solution. The answer to this question cannot profitably be given in terms of traditional categories, since an answer in these terms could hardly avoid commitment to a particular point of view. To be satisfactory, the general problems should be stated formally, leaving the character of their solution entirely open and undetermined. Systems of psychology might be written from any one of many points of view, but each might be expected to come to terms with certain common general problems. Such problems constitute the formal criteria of a system as viewed from within the subject-matter of the field. They constitute, thus, intra-systematic criteria.

This paper represents an attempt to state these formal criteria. It is based upon a study of actual systems and upon the body of psychological fact, not upon *a priori* considerations. It aims, however, not at an evaluation of specific systems and not at the presentation of a new one, but rather at the formulation of the general criteria in terms of which old systems may be evaluated or new ones constructed. It is believed that a way to some agreement among the abundance of contemporary viewpoints may be facilitated by an effort to state the basic common problems of system.

With the extra-systematic criteria this paper does not purport to deal. The definition of system given at the beginning of this section, which only states what seems to be the usual meaning of the term, summarizes certain of the more obvious examples of such criteria. They in turn presuppose others, and one must travel the long road of logic and scientific method, fortified with the credentials which application of the intra-systematic criteria can supply, if he would evaluate adequately the external criteria of system. In one sense, the two sets of criteria are closely interlocked, but a system must first achieve a degree of internal structure

before its external relations can be clearly envisaged. The formal standards for the evaluation of extra-systematic relations have been, moreover, much more frequently discussed at that level than have those for the evaluation of the intra-systematic organization.

Eclecticism and System

The last few years have seen the appearance of several champions of eclecticism who propose to erect a psychology either upon the pragmatic sanctions of history or upon whatever will satisfy the pragmatic demands of the phenomena as one immediately deals with them.[1] The eclectics seem to believe that a system must be *a priori* and fundamentally metaphysical. Yet as one reads the eclectics themselves, one is struck by the fact that what they are trying to do, in their escape from the bonds of existing systems, is to erect another, less *a priori* and less 'finished,' perhaps, but still a system, out of the principles which have been found to be most adequate in preceding systems. To the extent that one tries to order the facts of any field, system, in the sense of this paper, cannot be avoided. It is better, therefore, to attempt to systematize admittedly and rationally. One need not, as a result, found a 'school' or nurture a cult. It is possible to be a *psychologist*, not a defender of a school, and yet to attempt to order constructively the field one studies. The formal criteria of system which are to be described constitute an attempt to formulate the general problems of psychology, as it now exists, in a fashion which will avoid the traditional categories, encrusted, as many of them are, with the meanings of particular 'schools,' and which will commit to one solution no more than to another. In one sense an eclecticism is implied, but it is an eclecticism which proposes frankly to admit what *a priori* grounding it cannot avoid, as in the end all reflective science must, and then to erect as unified a structure as may be, expecting hourly, albeit, that

[1] See, *e.g.*, E. G. Boring, Psychology for eclectics, Chap. 6 in Psychologies of 1930; and D. B. Klein, Eclecticism versus system-making in psychology, Psychol. Rev., 1930, 37, 488–496. See, also, J. F. Brown's A note on Dr. Klein's plea for eclecticism, Psychol. Rev., 1931, 38, 182–185.

the then achieved unity may be broken by the next stubborn fact and must be recreated only to be dissolved again.

I. A Definition of the Field of Psychology

The first requirement of any system is a definition of the field which will clearly set it off from other fields and give it individual status and an independent set of problems. If this cannot be done, it must follow that psychology has no place as a separate field of knowledge and that systematization, *qua psychology*, is superfluous.

Definition was easier fifty years ago than now, although even then it was beset with difficulties. Then, however, psychology dealt with a conscious life which was generally regarded as unique and readily indicated. Since that day the data which claim psychological status have multiplied bewilderingly, far faster than systematic definition has kept up with them. Now a definition of the field must at least consider, if it does not include, data which range "from colour-theories to defence-mechanisms, from the functions of a white rat's vibrissæ to the mystic's sense of unutterable revelation, from imaginary playmates to partial correlations. . . ." [2] To include all relevant material and at the same time to mark psychology off from physiology and other related fields is a task not easily accomplished.[3]

It may be objected that definition is a scholastic formality over which a growing science will spill profusely before the ink is dry upon the words which define it. Such an objection assumes, however, a scholastic definition. What is necessary is not a sterile summing-up of a set of facts as if forever closed and static, but a definition of sufficient generality and flexibility of intent to give psychology a province and a criterion without committing it to unchangeable status. At

[2] G. Murphy, An historical introduction to modern psychology, N. Y., Harcourt, Brace, 1929, p. 1.

[3] This was the first problem attacked by Titchener when he set out to write his "final word on the establishment of scientific psychology, coordinate with biology and physics. . . ." (Systematic psychology: prolegomena, N. Y., Macmillan, 1929. The quotation is from Weld's preface.) The fact that the first volume of the projected work could justly be given over to definition is indication of the importance of the problem.

the boundaries there may be regions of vague sovereignty, but in so far as psychology is not something else it is definable and has a criterion of its own.[4]

II. Postulates

Coördinate and closely interwoven with a definition of the field lies 'the world of the postulates.' That every body of knowledge is grounded on postulational operations is almost trite, but that these operations need to be recognized and made explicit is a principle less commonly observed.[5] Such explicit recognition not only makes clear one's premises to begin with, which is necessary if a consistent structure is to be erected upon them, but it clears the ground for understanding and intelligent criticism. In systematic controversies one can find much discussion which would have been impossible had the 'schools' represented laid bare their postulates at the outset. One finds, also, some excellent cases of a shift in postulates in the middle of a book, a fact which introduces a logical rift in the system and contributes only to obfuscation. Explicit statement is a bulwark against such difficulty. Postulates there must be and small reason can be found for leaving them implicit and unremarked.

About the formal character of the postulates of a system little can be said without specifying particular ones and, while every system must accept a certain few, upon the others there is wide divergence of belief. On the side of formal character this much can be said with some hope of general agreement: that the postulates (1) should be necessary, not gratuitous or adventitious, (2) should be as few as possible, and (3) should include as little as possible of the finished system. Examples of both the fulfillment and the violation of each of these can readily be found in contemporary writing.

[4] W. S. Hunter's presidential address (The psychological study of behavior, Psychol. Rev., 1932, 39, 1–24) is a recent important attempt to define the province of psychology.

[5] The behaviorists have been the most active of all the exponents of 'schools' in laying bare their postulates. Cf. A. P. Weiss, One set of postulates for a behavioristic psychology, Psychol. Rev., 1925, 32, 83–87; and C. N. Rexroad, A formulation of the practical assumptions underlying psychology, Psychol. Rev., 1927, 34, 116–119. Members of other schools have, for the most part, preferred to leave their postulates for their readers to discover if they could.

III. The Character of the Data

Definition of the field and statement of the postulates will to some extent determine the character of the data which psychology will study. Such determination need not, however, be complete, and the degree of completeness will be a function of the specific definition and postulates. Inasmuch as the character of the data is not wholly given in the formulation of these two, it stands as an independent though overlapping problem.

Four general features of this criterion can readily be formulated on the basis of present knowledge. It must be established (1) whether the data examined are to be subjective or objective, or both; (2) whether they are to be qualitative or quantitative, or both; and (3) what are to be the units of description. Cutting across all of these is the requirement (4) that the units be such as to provide for a genetic starting point in the development of the phenomena studied and that they be equally valid at different stages in the genetic sequence.[6] Certain treatments of one or two of these features will determine the treatments of the others, but it is not necessarily so and the four have, therefore, been given individual statement.[7] The way in which they are handled will largely determine the methodology of the field. Nothing more need be said about the problem of method since its treatment is implicit in this criterion.

[6] Much, for example, of the disagreement between the protagonists of *Gestalt* and those of other 'schools' hinges upon the use of different units of description. In such discussions the problem of an adequate unit for genetic purposes figures largely. The same problem arises continually in writing on the nature of learning and in descriptions of the changes which occur in an act from the initial trial to the attainment of some level or criterion. The general problem of a unit is, of course, never absent from the discussion of any phenomenon.

[7] These features open the way for whatever kind and amount of experimentation the character of the data will permit. Implied in them, moreover, are all of the data to be studied, from whatever may be considered simple in any way to whatever is considered complex. The handling of the data and of the principles of organization, which are to be discussed later, might involve, for example, both a description of levels and of types, such as are found in current treatments of intelligence and of personality, and a description of individual differences and their relationships.

IV. A Mind-Body Position

A position upon the old mind-body problem has not been included among the postulates because, while the problem may be treated in this way, it may also be treated as an inference from data. By some this criterion of system will be styled too metaphysical for consideration. Its roots are deep in metaphysics, but it insistently rears its head, even in the midst of the laboratory, and must be recognized if an ordered body of knowledge is to be achieved. In certain of the contemporary textbooks we are treated to the interesting spectacle of a rather scornful disregard for this problem. The result, however, is that in one context the author implicitly uses one kind of mind-body solution, while in another context he deftly employs another.[8] This cool alternation has been defended (or rationalized) on the ground of pragmatic necessity, and from the standpoint of the interpretation of an individual experimental problem this defense may, perhaps, be deemed sufficient, but at the level of system it hardly contributes to logical order.

The problem need not, of course, be stated in traditional metaphysical fashion. The field may be so defined and the postulates so chosen that the mind-body problem becomes no more difficult than that of any interrelation between bodies of knowledge. The point is that the problem is there and must be treated (or avoided) somehow if an adequate system is to be constructed. It has much deeper ramifications into the specific content of psychology than the usual metaphysical discussions explicitly acknowledge. Whatever the data of the field are taken to be, so long as they are psychological, there exists the problem of their relation to the non-psychological.[9]

[8] Examples of shift of postulates, unmentioned alterations of mind-body position, and other violations of these criteria could be given from contemporary writing. A fair treatment of such examples would, however, involve one in a critical evaluation of current systems for which this is not the place. It will not be difficult for the reader to think of adequate examples.

[9] The relationships involved in theories of vision and of audition, in the data upon the relation between physique and intellect, in studies of cerebral function in learning, and in a multitude of others which may readily be thought of, are here in point.

V. Principles which Account for the Organization of the Data: Principles of Connection

After a specification of the data, no matter what their character, one meets at once the need of accounting for their interrelation. Not only are data organized cross-sectionally, but from point to point in the history of any individual, changes in organization are constantly occurring in bewildering complexity. All of this implies modes of connection, and the problem of discovering and formulating the principles (or principle) according to which organization occurs is crucial.[10]

These principles of connection must account for the whole universe of relations within the field of psychology, and must operate from the simplest connection between units of description to the most complex patterns in which the units may be found to arrange themselves. They must, of course, be adequate for both incremental and decremental changes in the relationships between whatever events are included in the system. The problem of principles of connection reaches into virtually every one of the traditional questions of psychology and its complexity is tremendous. Little can be said about it without indicating particular solutions, but it is a conservative judgment that an adequate and generally acceptable solution would set us far on the way to a satisfactory interpretation of a large number of the beleaguered issues of the subject.[11]

VI. Principles which Account for the Organization of the Data: Principles of Selection

It will, doubtless, be granted by most psychologists that principles of connection are, by themselves, insufficient to

[10] The term 'connection' is employed in a theoretically neutral sense in that it has no reference to any specific theory regarding the nature of such connections. In the sense in which the word is meant in this context, it might be used to refer to Thorndikian connectionism and to Gestaltist field organization with equal accuracy.

[11] Within the scope of this problem comes a large group of the traditional issues of psychology. Theories of association, much of the work on the conditioned response, learning in the sense of the connection and fixation of already selected units of description, some aspects of reasoning and, on certain views, the problem of meaning, are among the major questions which depend for an answer upon satisfactory principles of connection.

account for mental organization. Not all possible events within the field of psychology become connected in all possible ways within any given time. This may be stated on general logical grounds without assuming any particular definition of the field. The fact that some connections are made and that other possible ones are not, at once implies selectivity, and selectivity demands that principles of selection be found which will state why particular events occur and become connected while others do not. Selection is temporally prior to connection, but has been stated second for logical convenience.

In the present factual status of the subject, it is apparent that two kinds of principles of selection are needed. (*A*) One must account first for directional selectivity.[12] The data of any field which might possibly be designated as psychological may be expected to be in continuous change and this change must have direction. (*B*) The second principle must account for the intra-directional selection of individual and particular units of description. Individuals are not only oriented in particular directions, but certain specific items are selected, out of many possible ones, to enter into connection with each other.[13]

The principles of connection and selection combine to give a formal account of the organization (integration) of the data.[14] Some of the most significant, as well as of the most puzzling, experimental work of the present day is being done upon these two problems. Of all the formal requirements, they offer the most widely ramifying experimental possibilities, although the fulfillment of all, save the second, must be rooted in experimental fact.

[12] Directional selectivity involves the problems of attention, interest, motivation, *Aufgabe*, purpose, and the like, as they have been discussed traditionally. All of them have attempted to describe or to explain why the individual is pointed or oriented in a certain direction.

[13] This intra-directional selectivity includes the problem of selection in perception, learning and reasoning, and is obviously basic to the whole organization and character of the individual. It shares with directional selectivity some of the traditional problems listed in the preceding footnote.

[14] These criteria, as do certain of the others, admit of much further subdivision; but the particular subdivisions must be determined, not formally, but in terms of the content of the system which is being built.

Formal Criteria and Traditional Categories

The systematization of extant psychological facts, as well as of those whose discovery can be in any way foreseen, could be accomplished by a satisfaction of the six criteria listed. An adequate definition of the field and determination of the character of the data would at once determine whether the traditional categories, such as sensation, image, learning, and emotion, to mention only four, would be included and how they would be attacked, as well as the specific methodologies which would be considered valid. A statement of the postulates and of the mind-body relations paves the way logically for an organization of the data. The principles of connection and of selection account for this organization of the data within the field. The basic experimental work lies in the working out of the descriptive characteristics of the data and of the operations of connection and selection.

Many other and more general problems will be dealt with necessarily in the formulation of satisfactory fulfillments of the six criteria. Mechanism *vs.* non-mechanism, hereditary *vs.* environmental influence, instinct *vs.* learning and intelligence, and many others will be caught up into the working out of principles of organization. No system could be written without implied relationships to most of the problems of human knowledge and most of the general problems are basically extra-systematic. It should be remembered that the criteria suggested refer only to the internal problems of a psychological system. These criteria may overlap or even coincide, if certain points of view are taken, but whatever the point of view, all are problems which must be dealt with. Subordinate to each of them is a multitude of specific problems. An effort has been made to state only those which seem to be the most general intra-systematic problems and whose fulfillment, therefore, will constitute the criteria of any particular system.

Summary

An attempt has been made to state the formal problems of a system of psychology in such a way as to avoid commitment to any school or traditional category. By a system is meant,

not an *a priori* construct but a coherent and inclusive, yet flexible, organization and interpretation of the facts and special theories of psychology. Any particular attempt may be judged by the extent to which it treats these problems so as to achieve such a system. The problems thus become formal requirements or criteria of system.

The problems which have been considered basic are: (1) A definition of the field; (2) a statement of the postulates; (3) determination of the character of the data to be studied; (4) a mind-body position; (5) principles of connection, and (6) principles of selection.

[MS. received March 24, 1932]

THE FUNDAMENTAL PROPOSITIONS OF GESTALT PSYCHOLOGY

BY HARRY HELSON

Bryn Mawr College

A number of years ago I summarized the initial assumptions, definitions, and chief findings of the *Gestalt* psychologists in a set of propositions or 'laws' in an attempt to reduce their claims and important findings to as simple and clear form as possible.[1] Since then (1924) a number of psychologists who knew of the existence of this synopsis have requested it, although it has never been published. I now propose to give this set of propositions, in an amended form made necessary by the work of intervening years, for the following reasons: (1) to clarify a number of issues which have been confused, misunderstood or neglected in the literature, even by some so-called adherents of the movement; (2) to bring discussion of the issues to a more concrete, factual basis by presenting them as statements to be tested by an appeal to observation and experiment rather than logic and controversy; (3) to demonstrate that configurationism consists of a number of doctrines, (*a*) many of which are very closely interrelated, (*b*) some of which involve very different problems, and (*c*) each of which must be accepted, rejected or modified on its own merits; (4) to further experimental research on the questions involved by making the reader feel that he is here concerned with specific propositions capable of objective verification and not mere general phrases to be used on every occasion as catch-words, or never to be uttered except as anathema.

The propositions seem to fall naturally and will be grouped under the following seven headings: (1) What Configurations

[1] This was done in my dissertation, The configurational theory of perception, 1924, on file at the Library, Harvard University. Since then a number of other writers on the subject have used the same method independently, particularly R. Matthaei (see bibliography).

Are and Are Not; (2) Specific Properties of Configurations; (3) Types of Configuration; (4) Laws Governing Configurational Changes; (5) The Relation of Configurations to their Members; (6) The Relations of Members to their Configurations; (7) General Factors Favoring Configurational Phenomena. Final responsibility for the propositions as they stand in their present context must rest with the writer, whether or not they have been taken directly from various sources.[2] All shades of configurational opinion have been included while the attempt has been made to word the propositions so that they seem most acceptable in the light of all the evidence. A number of new propositions have been introduced which are based upon the writer's investigations of various aspects of configurational phenomena and names have been given to some of the propositions in order that reference to them may be facilitated, just as the word *Prägnanz* has come to stand for the more extended statement by Wertheimer given below. Illustrations are given of propositions which are not self-evident or too well known. Overlapping of material has been unavoidable because some propositions belong in more than one category.

I. What Configurations Are and Are Not

This topic concerns both matters of assumption, definition and empirical findings but it is beyond the province of this paper to attempt to distinguish between them. Nor can we divorce propositions concerning origin and development from those concerning the nature and properties of configurations.

1. Configurations are manifold, membered wholes. According to this definition absolutely homogeneous fields are not configurations. This view has been maintained by Höfler,

[2] It would be next to impossible to give an exact reference wherein most of the propositions as they stand could be found because they represent generalizations and summaries of extended discussions rather than a mere taking over of statements, except in a very few cases in which I have quoted directly. Moreover, it is difficult to determine just who was the first to come out with a given idea. Consequently I have resorted to the method of giving after every proposition the name or names of individuals most directly responsible for the idea contained in it. The works given in the bibliography at the end of this article contain the discussions upon which these propositions are based.

Krueger, Köhler, and Sander. It would be denied by Wheeler, for whom everything is formed.

2. A configuration is any segregated whole (Sander, Köhler).

3. Whatever is given is in itself more or less structured, more or less determined as a whole, having usually concrete whole properties and governed by inner laws (Wertheimer).

4. A configuration is a co-existence of phenomena in which each member carries every other and in which every member possesses its properties only by virtue of, and in connection with, all the others (Koffka). The important factor stressed in this definition of configuration is the 'membership' character of the parts.

5. To be configuration a totality must be demarcated from the rest of the field and possess distinguishable parts (Krueger, Sander).

6. Configurations are units arising against more diffuse backgrounds. This identifies the configuration too closely with figure as opposed to ground.

7. The first and simplest configurations are qualities on a ground (Koffka).

8. The first totalities are not membered or clear unities—configurations (*Gestalten*) are more or less embedded in totalities without definite structure, these totalities being largely affective in character (Krueger).

9. Configurations of all kinds represent dynamic processes in interaction (Köhler).

We next come to characterizations of configurations through definitions which tell what configurations are not and must not be confused with.

10. Not all totalities are configurations. (*Nicht alle Ganzheiten sind Gestalten*—Sander.) This means that we must distinguish between configurational wholes and mere totalities lacking the properties defining true configurations. I have elsewhere suggested that the term 'organic whole' be used for configurations and 'summative whole' for mere totalities.

11. Configurations are not sums of parts or parts and relations. Configurational perceptions are not perceptions of

parts-in-relation, nor are configurational reactions to be regarded as responses to parts and relations, as has been erroneously asserted by many writers in interpreting *Gestalttheorie*. (See sections V and VI for further discussion of this point.)

12. Configurations contain properties over and above the properties of their parts and relations. Thus when one perceives a square or a melody he perceives, if not more than the parts and their relations, at least something different from these. Perception of and reaction to parts and relations involve different configurations from those originally given.

13. Configurational properties do not arise from synthesis of parts in the usual sense. Nor are they the result of special processes like creative synthesis, *Produktionsvorgang*, attention, or entelechy.

14. Configurations do not depend upon parts or elements in the usual sense. Hence

15. Configurations are transposable regardless of whether the same or identical elements reappear (von Ehrenfels).

II. The Specific Properties of Configurations

This section concerns the differentiæ which distinguish configurations as such from non-configurational phenomena, if there be, as most writers on the subject believe, any phenomena which are not configurational in character. We are not interested in specific properties which distinguish some configurations from others (*e.g.*, tonal from visual configurations) but in generic differences.

16. Form is the most important property a configuration may have (Köhler). This tends to identify configuration with form and lays undue stress upon visual configurations which are usually formed. As Krueger has pointed out, other properties may be even more important for some types of configuration. Thus feeling totalities are characterized best by their intensity and their pervasiveness.

17. All configurations possess at least two distinguishable aspects, a figured portion called figure and a background called ground (Rubin, Koffka). Different properties attach

to figure and ground. In vision the figure is characterized by surface colors, the ground by film colors. Similar differences should be found for the figure and ground portions of configurations in other sense modalities.

18. Different configurations possess different degrees of *Prägnanz*, accentuation (Wertheimer), or articulation (Koffka).

19. Configurations possessing poorer structure are poorer configurations and lack properties such as impressiveness, clearness, and persistence. So-called attributive properties are results of configurational structure, while the converse is not usually true.

20. Every configuration either possesses or tends towards a certain order or end. This does not imply vitalism any more than the statement that "any alteration in the factors which determine an equilibrium causes the equilibrium to become displaced in such a way as to oppose, as far as possible, the effect of the alteration." [3] Many physico-chemical processes are thus seen to be configurations in this sense.

21. It is possible to specify what properties configurations must possess in order to be 'good' within a certain domain. For example, artistic configurations must possess strict unity, intentional breadth, and depth of composition (von Allesch).

22. Configurations are built upon some principle of organization which may be called the principle of coherence (G. E. Müller) or of structure.

23. Configurations may possess one center of gravity (*Schwerpunkt*) or several about which the whole is organized (Wertheimer).

24. Configurations tend to be meaningful or significant. The more meaningful of two structures tends to prevail (Sander).

25. Certain properties apply only to configurational

[3] This is the theorem of Le Chatelier as stated by F. H. Getman in Outlines of theoretical chemistry, John Wiley and Sons, New York, 3d ed., 1922, p. 298. Had this principle been formulated by a psychologist and applied to human or animal behavior we can well imagine some psychologists saying 'Vitalism, anthropomorphism.' The theorem of Le Chatelier recognizes that physico-chemical systems change in definite directions and enables one to predict in just what direction a system will change under given conditions.

wholes, like slender, round, and symmetry, while others apply equally to summative wholes, like simple, complicated, and regular (Köhler).

26. When different configurations are brought together to form a new whole, one may predominate over the rest or they may fuse. Thus a clang may be heard as a tone *sui generis* (complete fusion of partials) or any given partial may be singled out and it will predominate in the whole (dominance); after-images tend to take on the properties of the surfaces and objects they are projected upon, *e.g.*, appearing three-dimensional if projected upon solid objects or drawings in perspective.

27. Every configuration possesses an unique quality peculiar to it alone in addition to the general properties possessed by all configurations. An unique quality attaches to that succession of tones called a melody. It differs with different melodies and changes when the 'same' melody is played in different keys.

III. Types of Configurational Phenomena

There are several principles according to which configurations may be classified owing to the fact that configurations belong to several domains and may be regarded from different points of view. Without attempting to follow any strictly logical principle of division, which would require forcing into some preconceived schema, the following propositions are presented concerning the kinds of configurational phenomena.

28. Configurations occur everywhere. They are not confined to purely psychological phenomena nor to any particular class of psychological phenomena like perceptual data. There are physical, biological, physiological, psychological, logical and still other types of configuration. Whether or not we believe there is any distinction between these various types of configuration depends, largely, upon metaphysical assumptions that will not be considered here. As configurations, all possess configurational properties wherever found, although configurations in given fields possess special properties belonging to them as such. (See proposition 27.)

29. Configurations may be strong or weak, depending upon the degree of cohesion among the members (*Innigkeit*).

30. Configurations may be temporal or non-temporal. Temporal configurations require time for their completion, *i.e.*, they represent changing states in time. Non-temporal configurations either do not change in time or require so little time as to be, practically, non-temporal. Melodies are temporal configurations; a single chord is a non-temporal one. Dynamic processes, however, underlie all configurations.

31. Configurations may be 'good' or 'bad.' (These terms should be replaced by more specific terms denoting observable configurational properties like articulated, chaotic, non-membered, homogeneous, *etc.*)

32. With regard to structure, configurations may be homogeneous or non-homogeneous. If homogeneous they lack form and members, as in the case of an unbroken visual field or a continuous sound. Homogeneous wholes represent such absolute union or fusion of parts that no order or membership is discernible in them. It is questionable if such wholes should be called configurations or organic wholes. Non-homogeneous configurations may be firmly or loosely articulated (Koffka). If the parts are absolutely unarticulated we have, at this extreme, not a configuration but chaos (Matthaei).

33. Configurations may be open or closed, complete or incomplete. An instinctive reaction may be regarded as an 'open' configurational response until the consummatory response occurs to close it.

34. Different criteria of 'goodness,' 'completeness' and other configurational properties yield different results: a configuration that is judged 'good' according to one criterion may not be judged good with respect to another (Helson and Fehrer).

35. Configurations may be said to stand between two polar opposites: between summative aggregates and homogeneous, non-membered totalities. There are grades of 'gestaltedness'; there are degrees of insight. Configurational concepts do not represent absolutes.

36. Every configuration distinguishable in an organism like the psychological being is more or less an abstraction from the total configuration, the organism, made for purposes of emphasis and study, or to direct attention. With this in mind it is possible to isolate sensory, motor, sensori-motor, thought, memory, imaginative, instinctive and other configurations.

37. Certain terms refer to wholes or configurational processes even though they do not directly imply structures as such, *e.g.*, insight, gradients, structure-function reactions, reddening, and blanching.

38. Any process exhibiting direction in its changing states may be called a configuration (von Ehrenfels). Hence

39. Configurations are either dynamic or are the outcome of dynamic processes which underlie them (Köhler). Even the perception of such an apparently static phenomenon as a picture or a pressure is not to be regarded as revealing static wholes. Dynamic processes are at bottom responsible for stationary events of this sort. If sufficient time is allowed to elapse, the dynamic character of the underlying process may appear, *e.g.*, as one gazes at a patch of color it gradually loses in saturation and brightness, showing that the original stationary phenomenon was in reality 'quasi-stationary.'

40. Static configurations are rarely met with; most configurations are either dynamic or quasi-stationary; their changes are too slow to be observed except over periods of time (Köhler).

41. Configurations possess every degree of complexity both as regards their members and their mutual interrelations. We may speak of the customs, habits and practices of peoples, or groups of individuals as configurations in the same sense as applied to individuals.

IV. Laws Governing Configurational Changes

The last few propositions in the preceding section dealt with the dynamic nature of configurational phenomena. Further light is thrown on their dynamic character through a consideration of the laws governing configurational changes.

42. Configurational changes may be so slow as to escape detection or so rapid that their essential nature is overlooked. Illustrations have been given of slowly changing configurations (prop. 39) and the literature abounds in cases where configurational changes occur with extreme rapidity: gamma movement is an almost instantaneous phenomenon when observed without special arrangements; Adams has pointed out that insight involves very rapid changes. Configurations also change normally.

43. Configurational changes are subject to laws applying to them as wholes. This proposition is to be interpreted as a statement regarding wholes and changes occurring in them and not as one describing the way parts go together.

44. Configurational changes may be due to inner or outer forces; usually, however, they proceed from forces within the structure itself. In the latter case, the law of inner necessity prevails (Wertheimer).

45. Configurational changes are autonomous in the sense of obeying the law of the configuration involved (Köhler, Wertheimer).

46. Configurations change in some definite direction (Lewin).

47. Configurations tend to become as 'good,' precise, and impressive as possible. (Law of *Prägnanz*—Wertheimer.)

48. Configurations may change toward a more uniform, level state in which differences tend to be obliterated (Werner, Wulf).

49. Configurations tend to become as simple as possible. (Law of simplicity—Wertheimer, Köhler.)

50. Configurations tend to become complete in time. (Law of completeness—Wertheimer; law of closure—Koffka.)

51. Configurations tend to resist changes, *i.e.*, they tend to maintain their structure against disturbing forces.

52. Configurational changes occur in such a way that the time integral of the energy expended is a minimum. (Law of simplicity—Köhler; law of least action—Helson, Wheeler.) The law as stated in prop. 49 refers primarily to perceptual configurations, whereas as formulated by Wheeler and myself

it applies to behavior patterns as well, indeed to all configurational systems.

53. A change in one part or aspect of a configuration can be made only at the cost of or change in another. (Law of compensation—Helson.) Thus two objects of unlike size but equal weights are judged to differ in weight, the smaller being the heavier (size-weight illusion). Here what the larger gains in size it loses in weight; what the smaller loses in size it gains in weight, phenomenally.

54. One part or aspect of a configuration may take the place of or have such a profound influence upon another as to function for it. (Law of intervalence [4] or vicarious functioning—Helson.) In the *Tau* effect, shortening the temporal interval between stimuli (visual, auditory or tactual) results in a shortening of the experienced spatial interval; brighter objects are judged to weigh less than darker; objects that leave the ground at a higher speed are judged to be lighter than those leaving at a slower rate. In all of these cases the experienced change (usually called the 'illusion') carries all of the ear-marks of reality and can be explained only by the assumption of a unitary underlying process in which the various aspects exert real and mutual effects upon each other.

55. Relatively unmembered, unarticulated wholes tend to become membered, articulate wholes (Seifert).

56. Configurations tend to appear and disappear as wholes. McDougall and Rothschild [5] found that after-images obey this law.

57. Configurations tend toward symmetry, balance, and proportion when membered. This proposition applies to motor as well as perceptual and other configurational phenomena.

58. Configurations tend to appear in their characteristic,

[4] The term 'intervalence' was coined by Dr. H. M. Sheffer of Harvard University some years ago and more exactly denotes what might be called 'interchangeability.'

[5] W. McDougall, Some new observations in support of Thomas Young's theory of light- and colour-vision, *Mind*, 1901, 10, 52–97, 210–245, 347–382. H. Rothschild, Ueber den Einfluss der Gestalt auf das negative Nachbild ruhender visueller Figuren, *Arch. f. Ophthalmol.*, 1923, 112, 1 ff.

normal or average manner. Hence the constancy of size, shape, and color of objects under conditions wherein we should expect changes in these.

59. Configurations leave after-effects which make their reappearance easier under less favorable conditions. (Law of reproduction—Rubin, Koffka.)

60. If the conditions for the persistence of a configuration are not favorable, and a change to another configuration impossible, two simpler wholes will result. An example from vision demonstrates this law: normally, single vision is maintained as long as possible from the two retinal images; if single vision becomes difficult, squinting or suppression is resorted to to preserve singleness of vision. Last of all double images give rise to double vision. Depth perception represents the preservation of single configuration through complication.

61. Configurations may fuse, giving rise to new structures.

62. Simple configurations fuse more easily than more complex.

63. If more than one configuration is possible from a complex, the more meaningful tends to be realized (Sander). After the faces in the Rubin figure have been discovered they tend to persist rather than the vase. (It is possible to keep faces and vase simultaneously since both are meaningful.)

64. Configurational changes may be continuous or discrete. In learning the motor pattern may exhibit more or less gradual transformation in the direction of greater adequacy or may change suddenly.

65. Changes within configurations are the result of a texture of stresses (Köhler).

66. End-states may be as effective in determining change within a configuration as earlier states of the system (Köhler).

67. The reproduction of configurational responses within the organism follows the laws of habit, but the laws of habit are the laws of configurational change.

68. When several configurational forces are in conflict the stronger will prevail to produce its type of configuration. In this way *present* configurational demands may win out over

habitual or frequent configurational response. For example, a well-known figure may not be seen or recognized if it is enmeshed within a structure which does not favor its appearance. Here the new structure is seen or responded to instead of the habitual one owing to the stronger forces arising from the structure actually present, in this case the new one.

V. The Relations of Configurations to Their Members

The whole-part and part-whole problem raised anew by *Gestalt* psychologists will receive treatment in this and the following sections. It must be confessed that the issues here have been confused both by proponents and opponents of configurationism in their discussions of this topic. One of the issues raised is that concerning the possibility and validity of analysis and synthesis. In denying that analysis is valid or fruitful the configurationists, with their opponents, have failed to make clear that there are different kinds of analysis and synthesis. Certainly some sort of analysis must be resorted to, and parts somehow can be said to enter into configurational wholes. In the light of the configurationists' findings we shall have to grant that a new view must be taken of these issues. The following propositions indicate, I hope, a point of view which will prove fruitful in regard to these questions.

69. The locus of any configuration lies in the whole and not in any of the parts or parts-in-relation. This is really a corollary from prop. 27, wherein the uniqueness and novelty of configurations is stressed.

70. Configurations are prior to their parts and relations (Koffka). This proposition may mean a variety of things. It may mean: that the whole is prior to the parts logically or genetically or both; that form is prior to or determines content; that the gross precedes the fine or the specific, whether in perception or movement patterns. Concretely, it is easier to recognize a melody than a phrase in it; to recognize the phrase is easier than to recognize a chord; to recognize the chord is easier than to recognize a particular tone in it, and so on. And behind the tone, chord, phrase and melody lies

the still larger whole of non-auditory and feeling experience against which the auditory experience arises (Matthaei, Sander, Krueger).

71. Configurations cannot be analyzed into parts and relations. This should be taken to mean 'any, optional parts and relations.' It is therefore not in keeping with configurational doctrine to assert, as is so often done, that configurational responses are responses to parts-in-relation. Such responses are different from the responses to the originally given configuration and must be regarded as configurational responses of a different order.

72. Configurations can be analyzed in the sense that the necessary and sufficient conditions of their appearance must be given (Helson). Just as composition involves the creation of something new, so decomposition involves a loss. We may call this the law of decompositional loss.

73. Parts and relations are the result of a breaking down of given configurations into others. They may also be regarded as products of higher order than the originally given configuration because often it is more difficult and involves a higher level of accomplishment to react to parts and relations than the originally given whole.

74. Configurations condition their parts, not parts wholes. This may be referred to as the law of whole-part determination.

75. Any condition which affects the whole as such will modify the parts as well either in the direction of closer unity or toward independence of each from the other.

76. There are various grades of configurative unity and hence various degrees to which parts partake of whole properties. 'Organic' wholes are those in which a change in any one part radically alters the rest.

77. Maximum connectedness results in homogeneous totalities which are non-membered. It is doubtful if such wholes should be called configurations (*Gestalten*).

78. The more whole properties dominate, the more difficult to isolate or detect parts.

79. Whole properties do not depend upon parts; con-

figurations are therefore transposable whether or not the same parts are given, provided that configurational conditions obtain.

80. Configurations are responsible for connectedness; connectedness does not give rise to configurations.

81. Configurations change through accentuation or leveling of parts. Thus contrast is a configurative change through membering of parts, while adaptation is a change involving leveling of parts.

82. Special factors within configurations which determine part properties are numerous, among them the following: (1) position within the structure; (2) membership in the figure or ground; (3) relationship to the center of gravity (*Schwerpunkt*) of the whole; (4) the rôle played by the part in the plan, economy, centration of the whole; (5) in temporally extended configurations whether the part opens, closes or is a transitional member of the whole.

VI. The Relations of Members to Their Configurations

In the preceding section the propositions refer mainly to alteration of factors which affect the whole primarily and exert an influence on the parts more or less indirectly or through the mediation of the whole. There it was the whole which transmitted the changing forces. In this section we shall discuss how changing parts affect the whole. Whether there is any logical distinction between these two ways of regarding configurational changes we do not pretend to say. Certainly we can distinguish in practise between factors which act upon the whole, thereby changing the parts, and factors which affect parts with more or less effect upon the rest of the structure. If we change the illumination on a whole field we shall find that the parts of the field have undergone modification; but we may change the intensity or hue of a part of the field to see what effect this has upon the other parts or the whole. Experimentally we can proceed either way. We are now concerned with the second mode of approach.

83. Alteration of the parts changes the whole. By defini-

tion, the better the configuration, the more does a change in one part affect the rest. Changing the strength of the partials alters the nature of a clang.

84. A perfect configuration could suffer no change in any part without undergoing complete transformation. This proposition must be interpreted to mean within the limits of sensitivity of the reacting organism.

85. The perception of and reaction to parts and relations are different from perception of and reaction to configurations. It is possible to react to parts-in-relation. In such cases the configurational response must not be confused with the parts-in-relation response.

86. Parts producing configurations do so by virtue of conditions making for wholeness. If the conditions are right for configuration, the parts will 'go together' to form one, otherwise no mere linkage of parts will produce configuration.

87. Perception of parts hinders perception of wholes, and *vice versa*. This interdependence between parts and whole is mutual, inverse, and relative. We may call this the law of inverse correlation of part-whole response. Thus if one responds to the overtones he loses the clang, or if he responds to the clang, he misses the overtones as such.

88. The 'same' part may possess different properties in different configurations. Thus a gray of a given brightness appears lighter beside a black and darker beside a white.

89. Part properties may be accentuated or diminished when the part is made a member of a configuration. A shy individual appears shyer, a boisterous individual more boisterous in a company of 'average' people. On the other hand, an insane person seems less abnormal among other insane persons than in a normal community.

90. If parts are strongly directed towards each other, strong configurations result.

91. It is possible to determine what particular order or combination of parts results in the best configuration, regarded from any particular angle.

92. Any given combination of parts may give rise to several different configurations, depending upon the attitude,

interest, and other factors within the organism (Koffka-Kenkel). Ambiguous figures and so-called illusions furnish many examples of this proposition.

93. If one whole is made a part of another whole it may lose the properties it formerly possessed in its own right. When simple figures are embedded in more complicated structures it may be difficult, if not impossible, without aid, to find them (Gottschaldt).

94. The grade of importance attaching to any part of a configuration depends upon the part it plays in the whole. If it is the center of gravity, that part about which the whole is built, it will exercise an inordinate influence upon the rest of the configuration (Fuchs, Wertheimer).

95. If parts have occurred in a structure together and one of them re-appears bearing its membership character, it tends to re-instate the whole. (Law of reproduction—Koffka.)

96. Objectively equal parts may appear unequal in experience and objectively unequal parts may appear equal in experience. The Müller-Lyer 'illusion' offers an example of each case, for when the center lines are objectively equal they appear unequal in length, and when they are made to appear equal they are objectively unequal (Sander).

97. Parts having a poor position within a whole may be made more impressive by change of position or function within the whole. This proposition also holds true for groups of elements within configurations (Wertheimer). This proposition is readily applicable to social psychology and the psychology of leadership.

98. Parts which are alike tend to go together to form groups of wholes, *ceteris paribus*, rather than unlike elements. (Law of likeness—Wertheimer.) The likeness may be with respect to quality, size, position or what not.

99. Parts having smaller intervals between tend to form wholes rather than parts separated by larger intervals. (Law of nearness—Wertheimer.)

100. If several possibilities are present by which a part may be continued in a whole, the simpler, more regular will be chosen (law of the continuing curve—Wertheimer).

101. Any change in a part contrary to the general tendency of the whole will be resisted. (Law of common fate—Wertheimer.)

102. It is 'natural' to make order out of chaos, to connect the disconnected, to form structures out of groups. (*Tendenz auf Gestaltetheit*—Sander.)

103. With the tendency to form wholes goes a feeling of and for wholeness (Krueger, Sander). This feeling is more than a mere accompaniment of pleasantness. It is part of the dynamics of the totalizing process.

104. Individual differences exist with respect to the tendency to perceive and respond to parts *vs.* wholes. Several writers have suggested the possibility of analytic and totalizing types (Benussi, Rubin, Wertheimer, Werner, Helson, Fehrer and others).

105. There is a normal distribution of individual tendencies to react to configurational or part properties under any given set of conditions. That is, we may expect to find configurational responses following the same type of distribution that many other human and animal traits do. (Law of normal distribution of configurational response—Helson and Fehrer.)

VII. General Factors Favoring Configurational Phenomena

The factors influencing configurational structures have their locus both within the structure and in conditions outside it. The remaining propositions deal with factors responsible mainly for the appearance of configurations rather than for changes in them, a subject which occupied us earlier. We are concerned here with general factors influencing all, or a majority of, configurations rather than with the special conditions underlying configurations in given domains.

106. If the objective structure is strong, its appearance and capacity for evoking a response from an organism will be greater than if it is objectively a weak configuration. Sudden sounds elicit responses better than equally intense but less sudden sounds.

107. The attitude of the organism may aid or hinder configurational phenomena. If we are 'set' to see, hear or otherwise respond to a configuration we are more apt to do so than if not so set.

108. Attention, attitude, interest, habit, and other so-called subjective factors may be regarded as factors within the organism favoring or hindering configurational responses.

109. Dynamical interaction, undisturbed by accidental impacts from without, leads to orderly distribution of forces and hence configurations (Köhler).

110. Strong configurations are those in which inner as opposed to outer forces tend to predominate and determine the whole. Strong configurations have more chance of appearing and maintaining themselves than weak.

111. The general factors affecting physical, chemical, biological and psychological systems operate to determine configurations. Thus space, time, intensity, equilibrium, adaptation, survival value, goals, and purposes, to mention only a few, play a part in the type of configuration that appears. In other words, if we believe that psychological phenomena are in some measure related to and dependent upon biological, chemical and physical processes then the same laws must be active in the formation of structures wherever found (Köhler).

112. Configurations tend to be limited by space and time. This is particularly true of psychological configurations and has been described in traditional terms by such phrases as the 'span of attention.' A melody is bounded by time especially, while a visual form is bounded by space. Whether the former is also bounded by space and the latter by time I do not pretend to say. This may involve a real question or merely a pseudo-question.

113. Weak configurations remain weak in spite of attention, interest, and other subjective factors, while strong configurations do not need them to be impressive. This proposition is to be understood as applying within 'normal' limits only.

114. The stronger a configuration is in its own right, the

less does it require from the organism to appear and to elicit a response. It is not denied that favorable conditions within the organism may help to make a strong configuration stronger and to strengthen weak ones up to a certain point. If one has already 'learned' to enjoy Wagnerian music he is more apt to do so upon hearing it; but it should be borne in mind that if Wagnerian music were not 'enjoyable' in the first place no amount of 'training' or 'experience' or 'attention' would make it so. Learning consists really in discovery of configurational properties already present and requiring the 'proper' response.

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THE INSTINCT HYPOTHESIS *VERSUS* THE MATURATION HYPOTHESIS

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Classification of behavior as 'instinctive' and 'learned' has become within recent years conspicuously inadequate and noticeably invalid. Indeed, many psychologists no longer employ these categorical terms. The instinct hypothesis posits the unlearned character of certain tendencies; sometimes these are called inherited patterns, or innate proclivities —the particular terminology that is employed makes little difference. The basic assumption is that when appropriate environmental stimuli occur, certain stereotyped [1] responses will follow because original nature has provided mechanisms for response. Several years ago Bernard found in the literature 849 'instincts' which he reduced to 325. Just what the structural patterns are that could possibly elicit these reactions was never stated.

Among those who accepted the hypothesis there was difference of opinion as to the number and the nature of the instincts. Probably no competent student would have acknowledged the 849 instincts assembled by Bernard—or even many of the 325 'irreducible' patterns. Nevertheless, William James listed 28 instincts, and he included widely divergent activities such as *hiccoughing, crying, and hunting.* Thorndike had perhaps 46 in his catalogue, and Woodworth and others listed varying smaller numbers. McDougall believed that there were only seven (or nine) [2] primary instincts; he insisted also that each primary instinct was associated with a primary emotion. H. A. Carr revealed the utter disparity and confusion when he compared the lists presented by several psychologists (4, p. 380 ff.). Because 'instinct' lacks

[1] At least well-defined, well-organized, and nearly universal.

[2] McDougall sometimes listed seven, at other times nine or more.

clear-cut meaning some psychologists have for several years been reluctant to employ the term.

Several psychologists are today using 'maturation' in a manner similar to that which the instinct psychologists followed in employing 'instinct.' Allport writes:

> "It is . . . theoretically admissible that traits of behavior which make their first appearance during infancy, childhood, or youth may result from the 'ripening' or maturing of truly innate coördinations of reflexes, and not from experience. We may call this view the *maturation hypothesis*" (1, p. 44).

Although maturation has various connotations, it usually refers to behavior which is due to 'the normal process of growth'—behavior little or entirely unaffected by special practice or training.[3] It will be of interest to view impartially some of the methods which have been used recently to determine to what extent individual differences are due to practice effect and to 'the normal process of growth.' For convenience, these methods may be classified as follows:

1. The use of intelligence tests to disclose the influence of maturation.
    - *a*) The constancy of the IQ.
    - *b*) Correlation studies of near and far relatives.
    - *c*) Studies of foster children.
    - *d*) Studies of race differences.
2. Execution without previous practice: Maturation in animals.
3. Maturation experiments with students as subjects: Comparison of the performance of individuals after equal amounts of practice.
4. Studies of very young children.
5. Studies using the sequential method.
6. Studies of identical twins.
    - *a*) Co-twin control.
    - *b*) Identical twins reared apart.

[3] This usage recalls a term that was formerly used frequently in psychology, viz., the 'delayed instinct.'

1. *The Use of Intelligence Tests*

a) The Constancy of the IQ

One phase of maturation which has received widespread attention is that which deals with typical reaction levels established by intelligence testing. Binet's notable contribution, the mental age concept, enabled psychologists to identify rather accurately the mental status of a child, regardless of the child's chronological age, for Binet assembled in age groups *typical* reaction patterns of normal children. Goddard's revision of the Binet-Simon Scale, and Terman's extensive revision made it possible to describe somewhat objectively and accurately, the mental maturation of the American school child. And Pintner's important contribution, an assembly of performance tests, initiated estimating the mental maturity of deviates who possess speech or language handicaps. The practical value of all these tests (as well as the group intelligence tests) has been amply demonstrated.

Workers soon noted that the normal distribution surface characterized the grouped scores from the tests of elementary school pupils in every grade. After several years of experimentation, psychologists discovered also that typical school children tend to maintain their points of deviation from the average. And in addition, Terman, Hollingworth, and others pointed out that the mental ratio of the extreme deviates is relatively constant.

Several workers then hurried to proclaim the importance of *inheritance* in determining mental maturation levels. They maintained that the constancy of the IQ and the resultant distribution of intelligence indicated that individual differences in the mental test scores were the result of a mental maturation which depended primarily upon hereditary predisposition. In viewing the data obtained from intelligence testing, a dual interpretation is always possible, and there is no way at present to tell which is correct. Frank N. Freeman has discussed the constancy of the IQ, indicating how bias influences the interpretation thereof.

"In the first place, the IQ is only relatively constant. The average fluctuation on a repeated test is four or five points on each side of the initial score. This means that half the fluctuations are more than four or five points either way. Whether one calls this constancy or variableness will depend altogether upon one's point of view. In the second place, constancy in the IQ would only be evidence that the IQ is determined by heredity if the environment fluctuated enough to lead us to expect marked fluctuation in the IQ. It is probably more common for the environment of the child to be relatively constant throughout his school career. If this is true, constancy of the IQ might merely reflect constancy in the environment instead of the effect of inheritance" (11, p. 626).

The relative constancy [4] of the IQ is a fact, but the causal factors in effecting this condition are intricate, complex, and difficult to analyze. Maturation of hereditary predisposition is, without doubt, an item of importance in bringing about the relative consistency of the IQ. The quotation, given above, shows clearly, however, that environmental factors also are such important determiners of one's mental status that one surely could not validly state that the 'normal process of growth' is *the* cause of IQ constancy. It is apparent therefore that these results of intelligence testing have not helped much in solving the age-old problem regarding nature-nurture importance.

b) Correlation Studies of Near and Far Relatives

The possibility of a similar twofold interpretation is found in the fact that the mean intelligence test scores of near relatives differ less than do those of distant relatives. For years hereditarians have urged that, since, for example, the IQs of siblings show a higher correlation than do the IQs of cousins, the force of heredity is greater than that of environment in bringing about similarities. Obviously, the increased distance of relationship may signify that increased differences in environmental conditions also exist.

Another statistical approach indicates the dilemma to which enthusiasm may lead. The most extreme position is

[4] Or inconstancy (change of central tendency by about five points).

well illustrated by Thorndike who found the coefficients of correlation between the intelligence test scores of many siblings to be about + .60. "Comparing this value with the average sibling correlation, + .52, found by Pearson for physical traits, which are free from ordinary environmental influence, the inference is drawn that home environment is such as to raise the correlation between siblings from + .52 to + .60" (**34**, p. 40).

The inference that home environment is such as to raise the correlation between siblings from + .52 to + .60 depends in part upon the validity of the hypothesis that the intelligence tests measure intelligence *very precisely*. The inference is based also upon another doubtful hypothesis, namely, that mental abilities are transmitted in precisely the same way as are physical traits. No one has yet demonstrated the truth of the first assumption. And certainly, no one has presented *crucial evidence* that the second is valid. These two criticisms of the work of Thorndike are acknowledged by the editor of the *27th Yearbook*, and, undoubtedly, they must be clear to Thorndike. The criticisms, however, are of vast importance in a study of the nature-nurture problem, for much of the literature has little or no significance unless initial hypotheses prove valid.

One may conclude therefore that the correlation studies using intelligence test scores have contributed little to actual knowledge of the rôle of innate maturation and of environmental forces in effecting mental status.

c) Studies of Foster Children

Of considerable importance in this connection are two studies reported for the first time in the *27th Yearbook of the National Society for the Study of Education*. The first was made by Freeman, Holzinger, and Mitchell in the vicinity of Chicago (**36a**); the second by Barbara Burks, Terman, and others in California (**36b**). It is of interest that the two studies arrive at rather different conclusions (despite the fact that the studies agree that an average home environment may alter the IQ by five to six points). Freeman and his

colleagues insist that desirable environment will increase appreciably the mental ability of the child. Burks and Terman assert that environment has little influence upon the IQ. Indeed, the California investigators came to the conclusion that "The total contribution of heredity (*i.e.*, of innate and heritable factors) is probably not far from 75 to 80 percent" (**36b**, p. 308).

The diverse interpretations of similar data reveal that all of the work with mental tests have enabled us to ascertain *only the child's general mental level*, and to predict, within rather broad limits, his future intellectual development. This work has contributed little to our actual knowledge of the relative importance of nature and of nurture in determining maturation rate. The ability to estimate mental status of American school children is, of course, a clear social gain.

d) Studies of Race Differences

Rates of mental development have been 'established' for several races, since the advent of the intelligence test. When the results of the Army Alpha were first published, C. C. Brigham analyzed the scores of foreign-born recruits (**2**). He found that the more recent immigrants made lower scores than earlier ones, and he concluded that the recent immigrants were therefore less intelligent than those who came here previously. Various critics pointed to the vitiating effect of language handicap and interpreted Brigham's findings merely as an indictment of the tests which purported to measure intelligence. Needless to say, the controversy aroused widespread interest.

It is of interest that Brigham now states that one of his later studies upsets completely his earlier analysis of the samples of the foreign-born in the white draft (**3**). And Garth, who for years has been assigning IQs to the races, now admits that there is some doubt whether conventional intelligence test results are fair to Negroes, Mexicans, and Indians (**12**).

Most readers will probably agree that the so-called general intelligence tests are imperfect instruments. This, however,

is not the point that we wish to make. *Even if the intelligence tests were flawless instruments*, our thesis would still be valid, *e.g.*, the tests were devised as instruments for measuring mentality '*when, and only when*, those tested have had common experiences and similar interests' (9, p. 9).

The studies of intelligence test results for various races (which have been redacted by Garth, Pintner, and the present writers) reveal little that is conclusive concerning rates of mental maturation of different racial groups. Nevertheless, the studies disclose important and interesting facts regarding the intellectual status of racial groups. And these facts are undoubtedly of considerable value to those engaged in school work.

2. *Execution Without Previous Practice: Maturation Experiments with Animals*

One of the first important scientific studies of maturation was reported by Spalding in 1873 (31, pp. 282 ff.). Spalding did not accept the theory of certain of his contemporaries who held that behavior at various levels of development is the product largely of learning. He attempted to ascertain whether accuracy in the pecking of chicks is learned or inborn. He hooded 20 chicks as soon as they were hatched, because he believed that pecking was elicited largely by visual stimulation.[5] The chicks were kept hooded from one to three days; the hoods were then removed, and the chicks at first remained quiet. Within two minutes, some of them followed the movements of insects with their eyes, and within periods from two to fifteen minutes in length, they began to peck at specks and moving objects.

The accuracy of the pecking of the chicks was observed. In Spalding's judgment, the responses were somewhat well-integrated, and the pecking reactions were fairly accurate. Spalding studied also the flight of swallows, and he believed that his second study verified his first observations. Four

[5] Chicks get practice at pecking *before they are hatched.* They peck their way out of shells in a fairly efficient manner, and they undoubtedly gain some increments of accuracy in pecking.

very young birds were placed in wire cages where they could not exercise their wings, and were kept there until they were old enough to fly well under normal conditions. They were then released; two of the birds flew in excellent fashion. The others flew with difficulty. This experiment corroborated (for Spalding) his theory that flying was due largely to maturation of the structure necessary for flight.

The student who believes strongly in the potency of practice will doubtless assert that much depends on how the phenomena are interpreted, and which phenomena are selected for interpretation. For example, why did Spalding not focus his attention upon the two birds that flew with difficulty? Did Spalding not attend chiefly to those facts which served best to confirm his previously established belief? Certainly, an impartial interpretation would require some explanation of the fact that two of the birds *failed* to fly in an excellent fashion. If the failure were due merely to lack of maturation, that fact could doubtless have been ascertained by a more prolonged and elaborate experiment.

About forty years after Spalding's report was published, Yerkes and Bloomfield studied kittens kept in seclusion for different periods of time. When the kittens were several months old, mice were brought in. Although the kittens, in some instances, were somewhat slow in responding to the mice, they displayed (according to the report) well-coördinated instinctive behavior patterns (**37**, pp. 253–263). If one accept the conclusions of Yerkes and Bloomfield, one would thereby accept the hypothesis that certain complex behavior patterns are the result almost entirely of the maturation of structure.

Nevertheless, not all students agree with the conclusions of Yerkes and Bloomfield. Dashiell gives an explanation decidedly different from that of Yerkes and Bloomfield, interpreting the behavior of the kittens as *learned* responses (**10**, pp. 190–192). In the opinion of the present writers, Dashiell's interpretation is at least as plausible as is that of Yerkes and Bloomfield. Both interpretations are subjective in character.

Some of the best recent work is that carried out by Carmichael. He attempted to determine the nature and speed of vertebrates in acquiring the ability to carry out muscular movements (**5**, p. 51). He worked with the embryos of the frog and the salamander. The embryos were divided into two groups. One group (the control) was placed in a developmental dish filled with ordinary water. The other was put in a dish containing a solution of chloretone.

After the members of the control group had become good swimmers, the experimental group was taken out of the chloretone, and was placed in ordinary water. Within twelve minutes, the drugged organisms responded to external stimulation, and, in less than a half-hour, all exhibited coördinated swimming movements.

Carmichael concluded that "the intricate development of such interrelated structures as receptors, nerve trunks, central apparatus, and motor end-organs appears to be determined by functional stimulation within the organism itself" (**5**, p. 56).

One may ask logically whether the thirty minutes in which the drugged organisms were recovering from the effects of the chloretone might not be a period of extremely rapid learning. This theory, however, was disproved by later experimentation. The results showed "that the time required for release is not a period of learning, but rather an interval for the masking effect of the anesthetic" (**6**, pp. 43–44).

Carmichael experimented with salamanders also to discover whether precise and coördinated responses to light are learned. He divided the organisms, taken during the early embryonic stage, into three groups. Group I was placed in a dark room that was presumably entirely free from vibration. A second group was subjected to ordinary diurnal changes in temperature and light, and to incidental vibration. A third group was exposed not only to changes in temperature and light, but also to the vibrations incident to the operation of shop machines and tools.

After the salamanders in the second and third groups had developed sufficiently to swim and move freely, the experimenter entered the dark room in which Group I had been

placed and suddenly flashed a light. *Several* of the salamanders darted off just as did the other groups when they were exposed to a light stimulus. Carmichael says: "It seems that immediate external stimulation is not necessary for the development of those neuromuscular mechanisms which subtend behavior in the types studied" (7, p. 258).

It is important to bear several points in mind in connection with Carmichael's study: (1) Obviously, Carmichael did not prove (nor did he claim to have proved) that the salamander's behavior does not become *better coördinated* with practice, (2) In the higher organisms, there exist many thousand kinds of behavior. The conclusions of Carmichael are of course applicable (and are applied by him) *only* to the salamander and to the *particular kinds* of behavior that were studied. (3) It is conceivable that the salamander's swimming organs are so constructed that darting (or swimming) away is inevitable when many stimuli are introduced; that the darting reactions are largely the result of random behavior. The last hypothesis assumes that, as in Loeb's studies of certain bi-symmetrical organisms, light is painful. If this were substantiated, it follows that Carmichael's study proves only that the salamander's organs tend naturally to respond to stimulation. This is quite different from a conclusion which implies that salamanders without practice display highly integrated and skilled responses.

Coghill's studies of the development of *Amblystoma* have yielded results which emphasize once more the significance of the maturation of organic factors. Coghill summarizes his findings:

> "1. The behavior pattern develops in a regular order of sequence of movements which is consistent with the order of development of the nervous system and its parts.
>
> "2. In a relatively precise manner, physiological processes follow the order of their embryological development in the function of aquatic and terrestrial locomotion and feeding.
>
> "3. Behavior develops from the beginning through the progressive expansion of a perfectly integrated total pattern and the individuation within it of partial patterns which acquire various degrees of discreteness" (8, p. 36).

Coghill's results might be interpreted as support for the contention that maturation provides well-coördinated responses in organisms other than those with which the experiment was conducted. Indeed, certain psychologists have asserted that the results of Coghill's experimentation are applicable to human beings. The following experiment of Kuo suggests that application of experimental findings such as Coghill's must be made with extreme care.

Kuo reared kittens in a laboratory and observed them closely when rats and mice were introduced (**19**). The introductions were made under various circumstances. Some of the kittens had been isolated at an early age, and for the first week of their lives they were not permitted to know that a creature such as a rat existed. Some were given rats or mice for companions from the first. Still others were permitted, from the time they opened their eyes, to watch their mothers kill rats. And some of the kittens were reared as vegetarians, while others had a diet of meat and fish as well as milk and rice.

Of the 21 kittens that were raised in an environment wherein rats were killed, 85 percent killed rats before reaching the age of four months. Of the 20 kittens that were kept away from rats, only 43 percent killed rats during the same interval of time. None of the kittens raised with rats ever killed one of the rats with which it was raised. The experiment indicated that being a vegetarian does not make the kitten less competent as a rat-hunter and killer. Nevertheless, most of the vegetarian kittens did not eat the rats that they killed. Kuo states: "Our study shows that kittens can be made to kill a rat, to love it, to fear it, or to play with it."

The cat has a body mechanism which fits it for capturing small animals. Nevertheless, the cat's potential career as a hunter is modified by its life conditions. When kittens are reared in a ratless environment, the 'instinct' to kill rats does not appear.

Although Yerkes and Bloomfield found that kittens display well-coördinated 'instinctive' reactions toward mice, both Dashiell and Kuo believe that these reactions are largely

learned. The foregoing diametrically opposed interpretations are not cited merely in order that the present writers may state their own position concerning the matter. The writers wish, however, to emphasize the following facts:

The student of psychology is often reminded that the problem of the relative weight of nature and nurture as determiners of behavior is 'one of fact to be settled by the results of observations and measurements rather than by argument.' But the numerous and contradictory interpretations that are based upon identical experiments make it evident that 'facts' always demand interpretation, and that interpretations are subjective in character.

It is true of course that every investigator's conclusions are influenced to some degree by his preconceptions, his prejudices, his habits of thinking, and so forth. It is easier to conjure congenial interpretations than to change one's preconceptions. This tendency is well illustrated throughout the present article wherein opposite interpretations of practically the same findings are often cited.

The tendency to maintain one's habits of thinking is very widespread. Concerning two of the foremost psychologists of today (Thorndike and Spearman) a book reviewer once wrote:

> "Here we have then, two men, each formulating a hypothesis within a year of the other (1903 and 1904) and twenty-three years later (1926 and 1927) publishing books, the results of exhaustive researches which substantiate their original positions" (33, p. 24).

In reading the abundant literature reporting maturation experiments it is well to bear in mind the fact that interpretations of data are always subjective, that they are often contradictory, and that they often reflect the concentration of attention on certain phenomena to the neglect of others. It is desirable therefore to maintain a clear distinction whenever possible between an investigator's factual findings and his interpretations. Different observers are likely to view behavior phenomena in different ways, and many who accept

the accuracy and reliability of an investigator's data disagree vigorously with the investigator's interpretation.[6]

Within recent years a large number of maturation experiments have been performed. Many of these illustrate extremely careful work. But of those who have carried out this work, some have emerged as strong environmentalists; others have come forth as vigorous hereditarians. The cautious will refrain from generalizing until verification of basic postulates has occurred.

3. *Maturation Experiments with Students as Subjects: Comparison of the Performance of Individuals after Equal Amounts of Practice*

Some investigators have found that when equal amounts of practice are provided, individual differences are enhanced. This fact has been used to support the hypothesis that ability is due chiefly to the maturation of innate equipment, for writers assume that innate ability sets levels of *maximum* development for each individual in most traits. And most persons seldom utilize their abilities. Therefore, equal motivation and practice cause differences to become more pronounced and noticeable. There are many other factors which might be conjured to explain the phenomenon that equal amounts of practice accentuate differences. For example, the following is illustrative. Attainment is by no means directly proportional to *measurable* ability, even when opportunity is extraordinarily propitious. He who has most, gains most; and he of least ability, gains very little. With increasing ability, attainment is made increasingly varied and abundant. This is shown clearly in the conspicuous differences in the attainment of extremely bright children (IQ 130 or above), and superior (IQ 110 or above). These hypotheses seem, at first thought sound. But the question whether equal amounts of practice increase or decrease individual differences is by no means settled. H. B. Reed stoutly insists that individual differences are *decreased* by practice (**27**).

[6] Thorndike believed that animals learn almost entirely by trial and error and display little insight. But Koffka points out instances in Thorndike's own data of sudden drops in the learning curve which, he thinks, indicate insight.

In a doctor's dissertation Reagan summarized recently thirty-one studies. After considering the data from many approaches, Reagan concluded that ". . . equal training sometimes increases individual differences in achievement, but more often, it seems, it reduces these differences" (**26**). Reagan might well have added that in some instances equal amounts of training have no effect whatsoever upon individual differences. For example, practice with related materials sometimes does not alter materially functions such as: the ability to discriminate pitch differences, recognition of colors (color blindness), certain types of retentiveness, and so forth.

The task of answering the question regarding the effect of equal increments of practice is complicated by numerous items; among them are the following: the problem of measuring precisely changes in efficiency, the problem of determining what 'equal amounts' of practice are, and the difficulty in taking into account the permanence of new acquisitions.

One experiment seems to indicate that rate of tapping is determined largely by hereditary factors. According to Gates, practice does not permanently affect nor alter this function. Gates concluded from his experimentation that, for the tapping function, the rate of growth and the limit of ability that may be reached at any time are due primarily to innate factors; that *functions* may be inherited just as structures are inherited (**13**, p. 95). Curiously enough, Gates' conclusion is not corroborated by Müller's study of identical twins (**20**), nor by the studies of F. N. Freeman who concludes that, in rate of tapping, nature and nurture are about equally responsible for efficiency (**16**, p. 195).

Gates attempted to ascertain also the extent to which another simple function, auditory digit span, can be improved by intensive practice. He concluded that coaching results only in temporary improvement; that the ability is due largely to maturation of the organism (**14**). Jersild has recently extended this type of investigation, and for several functions his findings seem to support those of Gates (**18**, p. 702). However, experiments such as all these upon equal amounts of practice do not lead to a generally acceptable or

defensible premise regarding the rôle of hereditary and of environmental factors since the subjects were tested after they had been 'multitudinously conditioned,' and since the several reports do not agree precisely.

4. *Studies of Very Young Children*

Intrenched both in popular and in scientific writing is the concept that the young child is equipped by nature with elaborate repertoires for action that are released automatically upon appropriate occasions. Many of these inborn patterns have been alleged to be precisely organized, well-defined, and unmistakably identifiable. Recognition of the fact that some reflexes are very difficult to modify led some to the assumption that reflex activity is dependent solely upon birth equipment and subsequent maturation. The report of the investigators at Ohio State University precludes acceptance of this concept.

> ". . . stimulation of the lips of a newborn infant is followed by the sucking reaction in over 90 percent of the infants of a given age, but a stimulation of the cheeks, eyes, temperature, taste, smell, and so forth, will also produce it.
>
> That is to say, sucking is a specific reaction to stimulation of the lips, but it is a reaction to many other stimuli also. Shall we say that this fact makes it less specific? Of course, if we define specific to mean only that one form of reaction should follow one form of stimulation, sucking is not specific. . . . The fact that seems most pronounced in our own investigations is that any stimulus may release any reaction. This, of course, is opposed to any definition of instinct in the historical sense of the term" (**25**, p. 210–211).

The work of Weiss and his collaborators is supported by that of the Shermans. The Shermans state that, just as bodily movements improve in accuracy and adaptiveness, so also do the *reflexes* develop after birth. They assert that it is highly probable that even before birth development of reflex occurs (**28**, p. 62).

Examples of how the reflexes of the newborn infant change are numerous. An infant at birth does not respond when

the sole of his foot is pricked with a needle, but after about 15 hours, he reacts to the stimulus by withdrawing the leg (**28**, p. 63). Apparently the reflex response to this pain stimulus is not ready to function at birth; it appears after a certain maturity has been reached, and the reaction develops precision as a result of experience.

The Shermans cite many reflexes which develop with practice and growth. They find the reactions of the newborn child to specific sensory stimuli to be aimless and inadequate. Theorists have asserted that waxing and waning of well-coördinated responses occur in a uniform manner which parallel the organism's needs. Experiments show, however (according to the Shermans), that *not even the reflex mechanism functions effectively in the absence of a propitious environment and of appropriate exercise.* The Shermans deny therefore that responses "mature." They assert that the structure of the organism matures, and that the sensori-motor activities are perfected only through practice. Therefore, many responses usually attributed to organic maturation are in reality products of learning. The foregoing assertion is proved to the satisfaction of the Shermans by observations such as the following.

> "From the beginning the infant makes trial and error responses to stimuli and the adequacy of his final responses is dependent upon the frequency of such activity. When stimulations are given close together the development of coördination is much more rapid than when they are given infrequently and casually, as the infant receives them in the ordinary environment. This indicates that many of the responses usually attributed to organic maturation are in reality products of learning, since such a speeding up of adaptive behavior could not take place were maturation necessary for its growth" (**28**, p. 84).

These writers emphasize once more the force and potency of learning. They assert that reactions in the infant at first are chaotic and meaningless. Structure alone accounts for the variations among children in the first reactions that are made; and meaningful, adaptive behavior results from learning and from growth. And the doctrine which attributes learn-

ing to the development of a chain of reflexes is called sharply into question, because the reflex itself is not a basic, elemental, unlearned response. The Shermans therefore reject the concept of reflex once advanced by J. B. Watson, and they adopt an even more strictly mechanistic basis in accounting for development.

Nevertheless, the finality with which the Shermans write is by no means indicative that their results are indisputable, and their assertions irrefutable. Opposed to the conclusions are those of Shirley who employed the sequential method in studying developmental patterns in children.

5. *The Sequential Method*

Miss Shirley has suggested the sequential method as a means for determining whether behavior is due more to practice than to "the normal process of growth" (**30**). The ages of the first appearance of several traits were obtained for a number of infants. Median ages for the onset of each trait were then established. An orderly sequence was then set forth according to these median ages of development. Fidelity to the sequence was determined by comparing the development of an infant with the sequential pattern.

Study by means of the foregoing procedure revealed only a small number of variations from the established order.

> "Motor development sweeps on in an orderly course, apparently little influenced by the exigencies of time, place, and current fashions in child dress and child training" (**30**, p. 524).

Shirley found that motor control in infants begins headward and travels footward. Data were secured by examining 25 babies weekly and biweekly during the first two years of the children's lives. Shirley concluded that the regularity and the universality of this sequence indicate that motor control is largely a function of maturation.

> "From baby to baby the sequence held, not only in the broad aspects herein outlined, but in great detail. Furthermore, the babies of biographies and those studied by Bühler, Burnside, Gesell, and Jones kept to this sequence. Can an order which holds

so universally be attributed to training or even to spontaneous practice? Does not its conformity to the anterior-posterior growth law make the motor sequence a normal unfolding of developmental processes,—in other words, a function of maturation?" (29, p. 205).

Although the Shermans reported a similar sequence, they place a somewhat different interpretation upon it. They assert that the upper end of the body is more sensitive than is the lower part; hence, the upper part reacts more often. The Shermans offer several explanations for the greater sensitivity of the anterior portion of the body.

"First, the nerves for touch and pain have fewer synapses in the face and head regions than in the extremities. The threshold at the synapses is higher in the newborn infant than in the adult, as many impulses have not yet passed over the nerve tracts, and impulses which must pass over a great many synapses have less chance to complete the final path. Secondly, the distance which an impulse has to traverse is greater for the posterior end of the body than for the face and head.

"A biological explanation may also be given. Physiologists have shown that in many organisms *metabolic activity*, or the process of chemical change in living cells, is greater in the anterior than the posterior end of the body. Increased irritability and rate of conduction of impulses accompany this increased metabolic rate" (28, p. 70).

These conflicting reports show the necessity of exercising utmost caution when one reads the 'latest' maturation studies. The 'conclusions' will depend upon which phenomena are observed as well as upon the attitude of the observer.

6. *Studies of Identical Twins*

a) Co-twin Control

Gesell's studies are of especial interest because of his technique, that of co-twin control. His work led him to believe that "there is no such thing as utter randomness [7] in infant behavior" (15, p. 307). Innate factors, rather than environmental forces, account for a large part of organismic

[7] The term 'utter randomness' is a bit ambiguous to the present writers. From one point of view *nothing* happens in the finite world in an utterly random manner.

growth and the resultant behavior patterns exhibited at various stages in the development of the infant. Gesell's study of the development of prehension in a pair of identical twins led to the following comments.

"The refinement of his [the infant's] eye-hand behavior comes not by the alleged utilization of snatches of successful random activity, but by the progressive acquisition and consolidation of a hierarchy of behavior patterns which are the result of developmental decrements and increments rather than the stamping in or chaining of satisfying successful reflexes" (15, p. 309).

Observations of identical twins revealed such similarity in behavior that Gesell concluded that environmental stimulation alone could not possibly have accounted for the likeness. The similarity was observed in sitting posture, in behavior with cubes, and in stair-climbing. At the age of 48 weeks, one twin was helped to scale a specially constructed stair-case. Daily practice for six weeks made her expert in climbing. The other twin, having had no previous training, was confronted with the stair-case at the age of 53 weeks; she climbed to the top with no assistance. Two weeks training made her fully as expert as her sister had been at the end of six weeks training. Gesell assigned this rapid gain of the second twin predominately to the influence of maturation. However, the extreme environmentalist is dissatisfied with Gesell's conclusions:

"The behaviorists find it hard to accept such conclusions if the authors mean to imply as they seem to, that conditioning is of relatively slight effect in hastening the formation of different behavior patterns in infancy. The behaviorist would be the first to admit that muscular growth and strength (I confess I am not quite sure what these authors mean by maturation) in any complicated activity are undoubtedly factors in behavior. We would never attempt to teach a 52-week-old baby to drive an automobile nor to run a foot race. One would like to point out here though that the method of conditioning described in these experiments was quite amateurish in character. . . . Surely even the neophyte in conditioning would have used first T and then C as control in different but similar experiments. The conclusion that training on

simple sensory-motor responses at the age of 46 to 56 weeks is relatively without effect in comparison with maturation is rather startling to one sophisticated in infant conditioning" (35, p. 204).

Observation of cube behavior showed, according to Gesell, that, despite training, there was a daily change in pattern which could not be considered to result from learning. Gesell says: ". . . these patterns are basically under the stress and the regulation of the intrinsic organic factors of maturation. The very fact that there is a growth trend toward daily change of pattern makes the behavior less susceptible to stereotype and to conditioning" (15, p. 315). Finally, Gesell reported a close degree of correspondence in the emotional behavior of the identical twins, and again maturation was alleged to have been *the* important factor in producing the similarity. But the behaviorist remains unconvinced. J. B. Watson replies as follows:

"While these experiments are interesting, they constitute no proof that identity of patterns in these two youngsters was any closer than would have been the case had he investigated infants of the same age and kept under relatively the same environmental conditions, namely, residence at the clinic, provided the children had been of the same general weight and physical makeup and previous conditioning. So far as I have seen from the report, no controls were introduced in the experiment" (35, p. 106).

Another investigation similar to Gesell's is reported by Strayer (32). This study was made in the Yale Psycho-Clinic; the purpose was to compare language development that may be attained by special coaching with that contingent upon ordinary growth. The method was that of co-twin control, and the twins studied were those previously used by Gesell and Thompson. The identity of the twins was marked at all test periods. Twin T was given 5 weeks of vocabulary training beginning when she was 84 weeks old. Twin C received 4 weeks of training starting when she was 89 weeks of age. Twin C's vocabulary at the end of the 4 weeks of training (age 93 weeks) was seven words greater than T's at the end of her first 4 weeks of training (age 88 weeks); this,

however, was not as great as T's achievement after 5 weeks of training (age 89 weeks). At the end of the experiment Twin C had not caught up with Twin T. Three months later (with no training intervening) the differences had almost entirely disappeared. "The twins were sub-average and not all the special training was able to bring them up to the average vocabulary of children three and a half months younger than they." Apparently maturation influences not only the effectiveness of training in vocabulary, but also the general pattern of behavior. Training, although it cannot transcend the maturation level, is to varying degrees effective in increasing vocabulary. But here again is a generalization that is based upon study of a single pair of identical twins—obviously an insufficient basis for generalization.

6. *b) Identical Twins Reared Apart*

The real question at issue in Gesell's work follows: What differences would appear in identical twins if they were reared to adulthood in quite different environments? Although there is no unequivocal answer to the foregoing question, several studies which bear upon it have been reported. Müller studied a pair of identical twins that had been separated from age two weeks [8] until they were 18 years of age (**20**, p. 438). Both were reared in the same general region and in about the same social status, but they had different amounts of formal school work. One twin had only 4 years of school training, while the other completed high school and in addition took summer work in college. In spite of these differences in experience and formal educational training, both made practically the same scores on the Army Alpha and on the Otis Intelligence Test. On the Army test, the scores were 156 and 153. However, the fact should not be overlooked that the twins' responses on the intelligence tests were not alike throughout all sections of the test. J., the better educated, had a considerably higher score on those sections of the Army Alpha test dealing with abstract

[8] In the interest of accuracy it should be stated that Poponoe records the period of separation as 'from 2 *months* to age 18 years' (**24**, p. 142).

word relations, while B. scored slightly higher in arithmetic and in the section involving rapid distinction between synonym and antonym.

Although the total scores on the Army Alpha were quite similar, the scores were strikingly different upon non-intellectual tests of motor reaction time, association time, will temperament, emotion, and social attitude. Müller concluded that the differences on the latter tests were due largely to environmental factors. He writes: "this one case is enough to show that the scores obtained in such [non-intellectual] tests indicate little or nothing of the genetic basis of the psychic makeup" (**20**, p. 442).

H. H. Newman studied several pairs of identical twins reared apart (**21**, **22**, **23**)[9]. His first study of twins 'A' and 'O' yielded results very different from those reported by Müller. In mental age one pair of twins displayed a difference of 23 months. In emotional and temperamental characteristics the resemblance was close. Newman writes:

"On the basis of this one case alone, one might conclude that emotional and temperamental characteristics are more strongly inherited than purely mental traits. It should not be forgotten, however, that Müller's case points to an exactly opposite conclusion" (**21**, p. 54).

Newman's second pair of identical twins (Twins 'E' and 'G') revealed differences very consistent with those found for twins 'A' and 'O.' In mental age the twins "E" and "G" differed by 23 months. In all tests of emotional traits and of temperament the twins were remarkably similar. Newman describes them as follows:

"In many respects the twins 'E' and 'G' agree with the twins 'A' and 'O' of our previous paper and disagree with Müller's twins 'B' and 'J' in which there was remarkable mental resemblance and more than ordinary temperamental differences. . . . Which type of case is more nearly general, only further studies can decide" (**22**, p. 104).

[9] Since this manuscript was written Newman has published two additional articles on identical twins reared apart. These later articles have necessitated no important modification of our general conclusions. See the *J. Hered.*, 1932, **23**, 3–18, and the *Scientific Monthly*, 1932, **34**, 160–171.

The third pair of twins studied by Newman (Twins 'C' and 'O') were found to be mentally average and about equal (IQs 99 and 101), but they had 'entirely different personalities.' "So opposite is the impression made by them upon all the observers that we were forcibly struck by it from the beginning. Yet the only test that adequately expresses this obvious difference is the Pressey X–O test" (**23**, p. 165).

On the basis of his work to date, Newman concludes that it is impossible to generalize safely about the effects of environment and training upon the reactions of identical twins reared apart (**23**, p. 165).

The foregoing studies of identical twins reared apart are of extreme interest because identical twins have identical genes and their heredity is presumably identical. According to H. S. Jennings: "Any differences between the two members of a pair of identical twins are certainly due to some other cause than gene diversity" (**17**, p. 144).

After studying Müller's data, Jennings concluded that "environment and experiences of the individual have large effect on temperament, emotions, and social attitudes, producing marked differences in these matters, in individuals that are genetically the same" (**17**, p. 167). Summarizing his conclusions regarding both Müller's and Newman's studies, Jennings writes:

> "The results . . . agree thoroughly with the conclusion, practically certain on other grounds, that both the genetic constitution and the environment deeply influence mental and temperamental characteristics; and that effects produced in one case by genetic constitution may be produced in another case by environment" (**17**, p. 169).

Gesell, after considering the available data (including the findings of Müller and Newman) recently wrote:

> "It will be no small task for the downright 'environmentalist' to elaborate an architectonic system sufficiently ingenious in its mechanics to account for the marvelous and stable correspondence displayed in the mental development of human twins" (**16**, p. 198).

If, in the foregoing quotation, one were to substitute for the words, "downright 'environmentalist,'" the words,

"downright 'hereditarian,'" would the assertion be any less truthful?

Conclusion

In light of the foregoing evidence, what is one to conclude? The following conclusions seem to the present writers to be inescapable. Early investigators discovered that various kinds of behavior differ in the amount of exercise or practice which was required for smooth functioning. These investigators then attempted to classify reactions into two separate or opposed types, namely, innate and acquired. These investigators were thinking evidently in terms of bi-modal distributions. Of late years the general applicability of the so-called normal distribution surface has received verification in many behavior studies. Most forms of behavior appear to be mixed in type due to a mutual interaction of hereditary and environmental factors. The attempt to differentiate types therefore is no longer considered valid.

The proponents of the instinct hypothesis employ the same erroneous thinking as that which was used by those who sought to identify 'psychological types.' It is no more valid to classify all human behavior bi-modally—into learned versus unlearned—than it is to classify all individuals as tall versus short. A very few reactions are modified only slightly by experience, a very few others are due almost solely to experience. The vast majority are due to varying degrees both to heredity and to experience. *Which* reactions are 'instinctive' (modified only slightly by experience), and which are 'acquired' (greatly modified by experience) is today largely a matter of personal opinion. It could scarcely be otherwise. *Every* reaction of course so modifies the nervous system that seldom, if ever, are two reactions identical in the same organism. If the term 'instinctive' be conceived to encompass reactions not modified by experience there could be no such thing as "instinctive" behavior. The attempt to enumerate 'acquired' reactions is likewise hazardous. All learned behavior must have developed out of unlearned behavior, and any attempt to distinguish the point where the *unlearned* behavior became *learned* behavior is futile.

Watson's suggestion that the term 'unlearned' be substituted for 'instinctive' did not help matters greatly. The task of distinguishing 'learned' from 'unlearned' reactions is no less difficult than the earlier attempt to distinguish between 'instinctive' and 'acquired' behavior. Both attempts are due probably to thinking in terms of bi-modal distributions.

The fact should be kept in mind that, even if general agreement were attained among the students of behavior regarding the number and kind of instincts (and there is little reason for assuming that such agreement could be attained), *any attempt to establish points of division along the base line of a normal distribution surface will be a purely arbitrary procedure.* If a single reaction of a single species were all that were involved, it is conceivable that a division point might be established by general agreement. When one recalls, however, that each species is capable of making thousands of reactions, and that the number of species is enormous, mutual agreement seems hopeless.

In their zeal to abandon and deny the term 'instinct,' many are today substituting the term 'maturation.' Perusal of the contradictory findings indicates that the term maturation is proving as flexible and as obscure as was the term instinct. J. B. Watson states that he is not quite sure what is meant by the term maturation (**35**, p. 204), and Pratt, Nelson and Sun wrote that when we say that well-coördinated reactions appear as a product of growth and maturation, this is merely a restatement of the fact that they do appear (**25**, p. 210). In some instances the use of the term 'maturation' certainly suggests strongly the phenomenon that once was labelled 'delayed instinct.' If Weiss and his co-workers found themselves unable to isolate the inherited factors in even so simple a sensori-motor mechanism as sucking, the student of psychology may as well admit frankly that he is unable to isolate the inherited factors in any complex human mechanism.[10] At any rate, as long as contradictory reports continue to stream from the printing presses, the cautious

[10] Unable, that is to say, to prove to the satisfaction of his colleagues that he has succeeded in doing so.

will probably find it judicious to postpone whole-hearted acceptance of the testimony of the recent enthusiasts who appear to substitute 'maturation' for 'instinct.'

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MEANS–END–READINESS AND HYPOTHESIS —A CONTRIBUTION TO COMPARATIVE PSYCHOLOGY

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Each of the present writers has proposed recently and independently a concept to define and characterize certain *selective* features in behavior. An animal when presented with any given situation does not respond to all the stimuli composing that situation nor does he release the whole repertory of behavior of which he would be capable. He responds to certain of the stimuli only and to them only by certain of all his possible behaviors. Tolman (**9**) in considering this phenomenon proposed the concept of *means-end-readiness* and Krechevsky (**5**, **6**, **7**) that of *hypothesis*. As a result of mutual discussions the two of us now feel that these different concepts were very similar. We also feel that it would be illuminating to state in just what ways we both have now agreed to use them in the future.

By way of introduction let us point out very briefly the ways in which we did use these two terms. The 'means-end-readiness' Tolman defined as a certain selectivity as regards stimuli, and as regards the responses to be made to such stimuli, which the animal, because of innate constitution or previous training and as a result his physiological demands at the moment, brings with him to any given stimulus-situation. Thus to take a concrete example, Yoshioka (**10**, **11**) reported that hungry rats when presented with large and small sunflower seeds tended to select (for eating) the larger seeds. It appears further, however, that such selectiveness was contingent upon hunger and also upon the large seeds actually satisfying hunger better than the small ones. When the rats were not especially hungry they did not select more of the large seeds. And when after much practice it

was, so to speak, 'proved' to them that large seeds took longer to eat and gave no bigger meats than small seeds this selectiveness likewise disappeared (**12**). The means-end-readiness was in this case defined therefore as a propensity for a given selected kind of performance (eating large seeds rather than small) which the hungry rats brought with them to the given stimulus set-up and which they modified because actually this selection of the larger seeds did not prove beneficial.

And the 'hypothesis' as defined by Krechevsky was similarly a selectivity as regards type of stimulus to be responded to and types of response to be made to such stimuli. In an analysis of the learning behavior of rats in a four-choice successive discrimination-box, Krechevsky was forced to conclude that this learning process is not to be described as consisting of two periods, the first a period of haphazard and random responses and the second, a period of unified systematic behavior. Rather he found that the whole period of learning involved a succession of systematic behavior patterns, one after another. And on the basis of various lexicographic considerations he proposed the term 'hypotheses' to designate these successive systematic patterns.

Here again a concrete example will facilitate the elucidation of the concept. When the rat was placed in the discrimination-box his behavior from the first day until the day he had established the habit could be shown to consist of the following types of pattern. First, the pattern was very likely to be a spatial one. That is, the rat was very apt to enter the right side (or left side) of the box consistently in practically every trial. But then, after persisting in this type of response-pattern he would usually drop it and assume, say, an alternating pattern, entering first the right side of the box, then the left, etc. Finally, he would give up either of these patterns and adopt the 'correct' one of entering every lighted box. *From this point on, he would show no more variation as long as the lighted box continued to prove the 'correct' one and to lead to food.*

The term 'hypothesis' then, in the above type of case, was used to characterize any of the following: *A*) The rat's con-

sistent right (or left) position responses, *B*) his consistent alternating responses, and *C*) his final consistent light responses. And this term hypothesis was chosen because it suggested the following four *behavior* characteristics which were found in all these sets of patterns: 1) the systematic character of the behavior; 2) its docile [1] character (*i.e.*, the fact that the animal soon gave up one of these modes of response when it did not lead to an efficient attainment of the goal; but he persisted in it if it proved a 'good' form of response); 3) its selective character (the animal, during any one period responded not to every stimulus confronting him, but only to certain ones); 4) its 'self-initiated' character (it appears from other experimental evidence that these various modes of behavior did not depend upon the immediately presented situation for their initiation). In other words such patterns of response were innate, or the result of previous experience (7).

It is obvious that the two concepts have much in common. Before, however, proceeding further to state our present decision as to the future uses to be made of them it may be well to point out more clearly what is really the methodological status of both. Such concepts as these of means-end-readiness and of hypothesis (and, indeed, many similar concepts) may be phrased in either of two ways. On the one hand, a means-end-readiness (or similar entity) may be conceived of as a separate variable or determinant which intervenes between stimuli and responses and which thereby helps to explain why the given individual response follows from the given stimulus. On the other hand, and with perhaps less danger and greater fruitfulness, it may be pointed out that a 'means-end-readiness' or an 'hypothesis' (or 'desire' or 'conditioned-response connection' or what not) is also but a special value or form of the general *relation* obtaining between stimulus-situations (together with, of course, the physiological condition of the animal) and given succeeding responses. That is, an hypothesis or a means-end-readiness (or the like) is the *name* for an observed relatively specific

[1] For this use of the term 'docile' see Tolman (9), chapter I.

type of correlation, or equation, holding between stimulus-situations and resultant response-patterns. When such a special form of the equation is observed to obtain, one says that the animal has such and such an hypothesis (means-end-readiness) and one predicts that as long as the animal's internal conditions remain unchanged this same special form of equation between the given stimulus-situations and the resultant response-patterns will continue to be exhibited. It is important thus to emphasize that the special form of the stimulus-response equation which one calls an hypothesis (or a means-end-readiness) is something which depends upon factors *in the organism*. There are, that is, conditions in the organism arising out of innate endowment or past experience which determine the appearance on the particular occasion of the given hypothesis or means-end-readiness. Thus one individual of a given species may, when presented with a given environmental stimulus-situation and a given physiological drive, exhibit one limitation (hypothesis, means-end-readiness, desire, conditioned-response connection or what not) of the more general stimulus-response equation holding for the species as a whole; while another individual of the same species (or the same individual at a different time) will, due to differences in make-up or immediate history, exhibit a different limitation (hypothesis, means-end-readiness, desire, conditioned-response connection) of this same general stimulus-response equation.

Thus, it is to be strongly impressed upon the reader that all such terms as these of 'desire,' 'aversion,' 'expectation,' 'hypothesis,' 'means-end-readiness,' are merely names, or concepts, for relationships between variables. They do not say anything about the *vera causa* of these relations. The psychologist, just as the modern physicist, contents himself with stating functional relations existing between his data. Beyond that he does not go. Our description of the possible uses of the concepts of means-end-readiness and hypothesis is to be understood in this sense only.

Examining again the ways in which the two terms were originally used by Tolman and Krechevsky, we note that they

seem to have been almost identical concepts. Let us reiterate their similarities.

The defining aspects of both means-end-readiness and hypothesis appear to be the following *stimulus-response* facts: 1) an animal in any given situation does not respond to all the stimuli present nor does he release his whole quiver of responses, but at all times his behavior is systematic and selective as to stimuli and response-pattern; 2) this selectivity of stimuli, and of responses relative to them, varies with the experience of the animal in the given situation so that in the long run it would appear that this selection is a *tentative* one and that it changes relative to whether or not it results in the animal's reaching successfully and easily some final goal-situation (*i.e.*, the selectivity is 'docile'); 3) the initial selectivity is one which apparently does not depend entirely upon the experience of the animal in the immediately given situation, but is one which he has brought over from some previous situation, or it is, perhaps, innate. In other words it is 'self-initiated'; 4) such selections also depend upon the motivation conditions of the animal in the sense that those selectivities which an animal originally exhibits and the changes which appear in them both hinge upon his motivating conditions; so that, for example, when hungry he will exhibit one set of selectivities, and will show modifications in these selectivities of one sort; whereas when sexually excited he will exhibit a different initial set of selectivities and will exhibit a different direction of modifications in them.

But, if these two concepts—means-end-readiness and hypothesis—are thus so much the same, why, it may be asked, keep them distinct? Our answer will be to suggest that these two terms may from now on be used respectively for two functionally different types of instance of the one phenomenon. This proposed difference of usage will become clearer if we consider again our cases of learning. For it is primarily in the learning or problem-solving sort of situation that such selectivities make themselves obvious. And further, it must be noted that in such learning set-ups there appear always two, and sometimes more, levels of selectivity:—wider, more

general levels and narrower, more specific levels within these wider ones. And our suggestion is now going to be that, in considering *any two* such levels, the term means-end-readiness may be used for the wider and more general level of selectivity, while the term hypothesis may be reserved for any one or more narrower, more specific, selectivity appearing at the next level and within the range of this wider selectivity. But to make this clearer let us return again to the examples cited above.

In Tolman's example, taken from Yoshioka, it is to be noted now that the rat, when presented with the seeds, proceeded first of all and most generally to eat them. This fact that, when hungry, the animal systematically set about eating the seeds instead of systematically treading over them or ignoring them and gnawing at the boards of the box, or trying to chew the wire of the apparatus, we shall now call the means-end-readiness. This means-end-readiness was relatively general and one which the rat probably brought to the situation innately, but in any case it satisfies the criteria. This eating of the seeds shows a true degree of selectivity in relation to the stimuli and to the type of response to be made to them. Secondly it is to be pointed out, as previously noted, that the animal not only showed this 'gross' selectivity (reacting to seeds rather than to the dozens of other possible items in his immediate environment) but also that he exhibited a further more restricted selectivity within this more general 'seed-range.' He tended to select the larger seeds and to eat them rather than the smaller ones. And this latter narrower selectivity—larger seeds rather than smaller—we shall now designate as an hypothesis which appeared within the wider means-end-readiness. Finally, however, in this particular example, it is to be noted that the learning itself consisted in the giving up of this latter hypothesis. For, after a great deal of repetition of the set-up, the rats finally ended by eating just as many small seeds as large. Learning, if we may call it such, consisted in this case, therefore, in the giving up of an initial more specific hypothesis and a reversion to a more general means-end-readiness. Turn now to the other example.

Krechevsky's rats when first put in the discrimination-box spent considerable time in trying to climb through the wires, in examining the restraining wall, and the like as well as in running in the alleys proper. But they did not lie down and go to sleep. They exhibited, in short, a very general selectivity of the general *exploratory* variety, and this we shall now designate as the means-end-readiness. But within this general exploratory means-end-readiness they also exhibited these various more specific sub-varieties of which running in the alleys was the most interesting to the experimenter and objectively most successful in reaching food. Such running in the alleys we shall designate therefore as one of a group of alternative hypotheses all of which appeared within the initial more general means-end-readiness of exploring. The first stage of learning seemed to consist in limiting the general means-end-readiness (to explore) to this single more specific hypothesis (to run in alleys).

Next, however, it appeared that the animal soon began to run not in any alley but rather to select all the right-hand alleys only, or all the left-hand alleys, or alternate right and left alleys, and the like. At this stage we shall designate, therefore, the general running of alleys (instead of trying to climb through the wires, etc.) as the means-end-readiness; while these more specific patterns of choosing all right alleys, or all left alleys, etc., we shall designate as hypotheses. Finally, the last stage of learning seemed to consist in the selection of one of these subordinate hypotheses (running in the lighted alleys) and the reduction of the more general means-end-readiness (to run in any alley) to this single more specific form.

It appears, in short, that at the beginning of a learning problem when the animal is introduced *de novo* to an apparatus or other problem situation he will exhibit a relatively wide range of stimulus-response possibilities. This widest range we shall call a means-end-readiness. But within any such general means-end-readiness there usually appear one or more specific alternative limitations. Each of these limitations we shall call an hypothesis. Learning may now be

phrased as consisting of the reduction of a more general means-end-readiness to some more specific hypothesis. The whole process may involve a succession of such more specific reductions, *i.e.*, from means-end-readiness to hypothesis and then from the latter (now called a means-end-readiness) to some still more specific hypothesis, and so on. Less frequently, however, learning may appear to consist of just the reverse—the giving up of a more specific hypothesis (in cases where for some reason one of them has already become dominant) and reverting back simply to some more general means-end-readiness (see Yoshioka's experiment, cited above).

Learning, of the general trial and error sort we are considering, is thus either the reduction of a means-end-readiness to a more restricted hypothesis (or a succession of such reductions) or it is just the reverse, the reversion back from a more specific hypothesis to a more general means-end-readiness. It is the former which is probably alone properly to be called learning; the latter would perhaps better be called 'unlearning.' A description such as this, implying the existence of *two* processes—'learning' and 'unlearning'—immediately suggests the further possibility that these two may have different sets of 'laws.' The conditions which favor learning—the reduction of a more general means-end-readiness to a more specific hypothesis—may well be quite different from those which favor the opposite process—the reversion back from a more specific hypothesis to a more general means-end-readiness.

As regards the former process (the only one usually considered in ordinary discussions of trial and error learning) Tolman, in another place (**9**, chapters XXI, XXII, and XXIII), has recently argued at length for what he calls the doctrine of sign-gestalt formation—the doctrine, namely, that the selection of a more specific means (*i.e.*, the means which appears in the finally established hypothesis) is due to the formation of a more specific meaningful *whole* connecting this given type of response-pattern as *sign* to the given to-be-reached goal as *significate*. Tolman further suggests that the laws underlying such sign-gestalt-formations include those

with a truly '*gestalt*' tinge as well as those of frequency, recency and the like (in a modified form).

As regards the other process, that of 'unlearning' or reversion back from a more specific hypothesis to a more general means-end-readiness, the only considerations bearing upon this problem at all are, it would seem, to be found in certain recent discussions of a new phenomenon designated as 'fixation' or as 'behavior constancy.' Thus Gilhousen (**2**) has recently defined the case of 'fixation' as one in which the organism persists in a given specific route to a goal in spite of the fact that another alternative and better route has been opened up to him. In our terminology, the given hypothesis has become so dominant that a reversion back to the more general means-end-readiness and thence a progression to the new and 'better' alternative hypothesis has become relatively difficult. Ellis and Hamilton (**1**) have similarly defined a concept of 'behavior constancy' in which the organism persists in a given response-pattern relatively long after it has been made useless to him as a way of reaching his goal (food). As regards the laws underlying such fixation and behavior constancy or rather, in terms of our present argument, the laws which determine their opposites—lack of fixation and lack of behavior constancy (*i.e.*, 'unlearning' or the reversion back to a means-end-readiness from an hypothesis), we have as yet but few data. Only two sorts of experimental study are now at hand—that of Krechevsky and Honzik (**8**) indicating that in the rat excess frequency tends to 'fixate' a given hypothesis, and those of Hamilton and Ellis (**3**, **4**) indicating that cortical destruction similarly is conducive to 'fixation,' or, as they call it, to 'behavior constancy.'

Finally, in closing, it may be pointed out that this descriptive problem of the behavior of means-end-readiness and hypothesis in learning and unlearning is but one phase of the total problem as to all the causal conditions underlying the determination and appearance of means-end-readinesses and hypotheses. What, for instance, brings about the initial appearance of certain subordinate hypotheses rather than

others, and what are the causes of the alternations (such as Krechevsky (7) has demonstrated) between one subordinate hypothesis and another. All these are, in part, a matter of learning, but undoubtedly they are also, in part, a matter of innate endowment and of the peculiarities of individual structure. They suggest a whole series of future problems to be investigated.

To recapitulate:

(1) The terms means-end-readiness and hypothesis are to be used to designate *systematic*, *docile*, *selective*, *self-initiated*, limitations which appear in the general stimulus-response equation of a given organism. Only certain types of response follow from given stimulus set-ups. Further, these propensities towards such specifically limited types of response are shown to be *systematic*, *docile relative to goals*, *selective*, and *self-initiated*.

(2) These stimulus-response limitations tend to exhibit themselves in hierarchies of more general and more specific. As regards any pair in such a hierarchy we have decided to call the more general one of the pair the *means-end-readiness* and the more specific one an *hypothesis*.

(3) Trial and error 'learning' may be defined as the reduction of a means-end-readiness to one of its more specific sub-forms (hypothesis). The laws of such trial and error learning have previously been suggested by Tolman as those of sign-gestalt-formation.

(4) Trial and error 'unlearning' is to be defined, in contrast, as the reversion back from a specific hypothesis to a more general means-end-readiness. The laws making easy or difficult such reversions seem not as yet to have been specifically investigated (perhaps because the usual description of the learning process does not posit the problem) save for two sets of studies which have pointed to the concept of 'fixation' (or 'behavior constancy') for the case where such unlearning proves especially difficult. One of these studies indicated that excess frequency on the given hypothesis produces such fixation, and the other group of studies, that cortical destruction in the animal similarly tends to make for fixations.

(5) The problem of the ultimate laws for the initial causation and the later history of means-end-readinesses and hypotheses is thus, in part, the problem of the ultimate laws of learning and unlearning; but it is also undoubtedly likewise a problem of the nature of individual differences, *i.e.*, a matter of relatively innate individual constitutions. But, in either event, the need for many further investigations is indicated.

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AN EVALUATION OF THE CONCEPTS OF REFLEX AND VOLUNTARY ACTION

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An experimental comparison of certain characteristics of reflex and voluntary responses of the human eyelid (**22**) has shown that there are objective criteria which under certain conditions may serve to identify a reaction as reflex or voluntary, but that conditions may be established which alter the features of response to such an extent that the two groups become descriptively indistinguishable. In other words, the experiment has failed to discover any descriptive feature by which this reaction may be labelled invariably and infallibly as reflex or voluntary, and thus serves to confirm the widespread contention that the distinction is unjustifiable. The confusion which characterizes the whole question argues strongly for the abandonment of the classical concepts, but their very persistence in spite of repeated criticism suggests that the case has not been made clear. As a matter of fact, most psychologists and physiologists continue to make the distinction between reflex and voluntary activity even when they seek immunity from criticism under cover of new terminology or of quotation marks which often imply, quite erroneously, the possession of some wisdom accessible only to the initiated, and current experimental literature affords many instances of the persistence of this distinction. An important example is found in the recent work (**13, 15, 23, 16, 29**) which has dealt critically with the conditioned reflex by demonstrating its similarity to voluntary response. The whole significance of this approach evidently rests on the implication of a difference in reflex and voluntary action. It is pertinent, therefore, that the concepts be examined with the purpose of determining to what extent and on what grounds they are inadequate, and whether there is any justification for their continued use.

Since the outcome of such an evaluative procedure will depend to a great extent on the criteria which are used in making the distinction between reflex and voluntary behavior, we shall have to consider the value of the categories as they are delimited by the most important of the available criteria, which for the purposes of this discussion have been grouped under three broad classifications: (1) Differentiæ depending on differences in *descriptive features of the responses* themselves without any reference to their antecedents. (2) Distinctions based on differences in the *concomitants of behavior*, its *antecedents* and *consequents*. (3) Differentiation in terms of different *functional relations* that may be shown to exist between responses and their concomitants.[1]

Differences in Descriptive Features of Responses

Perhaps the simplest and most direct kind of response classification available is the first, which seeks its distinctions in descriptive differences in responses themselves. Such descriptions must be made up either of facts about the anatomy or structure of the reactor or about the latency, extent, direction, form or speed of the movement. Thus the distinction between striped and smooth muscle has had a relation to the reflex-voluntary dichotomy and one still hears physiologists refer to 'voluntary' muscle. Here are possible means of identification, but thus unrelated to other facts they are of little value to a science of behavior. Moreover, when movement is described and measured in its various dimensions, continuity is the rule; there are no obvious lines of cleavage. If, for example, we lump together any given measure such as the amplitude of reactions of the lid, regardless of antecedents, continuous distributions result and the wider the sampling of responses the less tendency there is to multimodality. There is evidence that even the latency of response, which has frequently been offered as a basis of distinction, is similarly distributed, if a broad enough sampling

[1] A distinction between the attributes of response and response correlates is, of course, an arbitrary procedure which may be justified chiefly on practical grounds. The problems involved are beyond the scope of the present discussion.

is obtained and plotted without regard to antecedent conditions. The latencies which Wiersma reports (**30**) tend to confirm this statement, and our own results (**21**, p. 19) suggest the same fact. Similarly, the physiological work on frequency of action currents in reflex and voluntary reaction reveals no descriptive basis on which a dichotomy could be based.

Differences in Response Correlates

It is not strange, therefore, that purely descriptive differences in responses have never been used as the exclusive basis for the classification of action. Even Aristotle, who in his oft-quoted statement illustrates voluntary and involuntary motion by descriptively different responses, invokes additional criteria of distinction, when he says (**2**, p. 239), "I call, however, the involuntary motions, such as those of the heart and genital parts. For frequently in *consequence of something* appearing they are moved; though the *intellect* does not order them to do so—and such are the motions of sleep and wakefulness, respiration and others of like kind" (italics ours). It is the origin of the behavior itself, in which he is most interested here and particularly in the *Ethics* where questions of responsibility are under discussion. In a similar manner most criteria of differentiation of reflex and voluntary behavior have rested chiefly on alleged differences in response determinants or consequents. The nature of the correlates suggested and the modes of distinguishing them have been exceedingly diverse, but for the most part the emphasis has been on the determinant rather than on the response, and we turn now to consider the limitations of the categories under this general mode of differentiation.

The most vigorous protest against the reflex-voluntary dichotomy has come from the behaviorists and other self-designated objectivists who, obsessed as they are with the ancient spectre of indeterminism, assume that the distinction between a reflex and voluntary act stands or falls with the once prevalent belief that the reflex has rigidly determined antecedents and that voluntary action is presided over by a free and undetermined will. Weiss's statement (**28**, p. 362)

is characteristic: "The freedom of the will is an illusion that is based upon the inability to discriminate the origin of the implicit reactions. To regard voluntary behavior or volition as a category of human behavior seems hardly worthwhile. Volition has a social significance only if we assume the existence of some psychological or mental selecting agency. If this is denied voluntary behavior is no different from any other form of behavior." Pavlov's (19) wholesale labelling of all forms of behavior as reflex has similar grounds, and Skinner's recent article (26) follows the same tradition, defining a reflex as "an observed correlation of two events, a stimulus and a response," and suggesting that the description of all behavior is adequately embraced by the principle of the reflex. By thus taking to philosophical grounds these writers neglect possible differentiæ of a purely observational nature which were inherent in even the earliest discussions. Determinism is an indispensable postulate of science, and scientific categories which violate this principle cannot be justified, but this fact does not dispose of the question of whether or not there are legitimate modes of classifying behavior at the observational level, and this is our present concern.

What, then, from the scientific point of view, are the most common response correlates which have been suggested as grounds for delimiting reflex and voluntary action?[2] It has been variously said that voluntary action is conditioned by consciousness, by intellect, *Aufgaben*, *Einstellungen*, etc.; that it depends on the functioning of the cerebral cortex; and that it is acquired behavior, *i.e.*, dependent on acquired neural connections. On the other hand, it is stated that the reflex is not conditioned by consciousness; that it is determined by a certain stimulus to which it is an invariable, predictable and uniform consequent;[3] that it depends on a synaptic nervous

[2] Fearing has summarized the modern concepts of reflex action (10) in Chapter XVI and the reader is referred there for references to individual views on the subject.

[3] The statement of invariability of reflexes has been refuted in various quarters. See Jenning's discussion of this criterion of the reflex in unicellular forms (18, p. 277) and Dodge, for the same problem in human beings (6, especially Chap. I). The place of the problem of variability will be considered later.

system; that it is unlearned, *i.e.*, the neural pathways are inherited; and that it is purposive and adaptive.

In evaluating these criteria, three questions must be answered. In the first place, when responses are grouped according to their correlates, how does this affect the distribution of response characteristics; are differences in correlates related in any degree to differences in response? Secondly, on what grounds can the correlates of response themselves be satisfactorily differentiated? Thirdly, can any reaction of the intact organism be said to have determinants which do not also affect other reactions?

In attempting to answer the first question, we are faced immediately with the difficulty incident to the fact that some of the criteria of reflex and voluntary response do not lend themselves to experimental verification. Take, for example, the statement that voluntary action is determined by consciousness, and that the reflex is not conditioned by consciousness. Aside from the questions of fact involved, you obviously cannot set up a reflex by means of an antecedent which it doesn't have. The principal determinants which are available for establishing responses in the intact organism so that their characteristics may be compared are: (*a*) the important phenomena as yet very inadequately described as consciousness, set, *Aufgaben*, instructions, etc.; and (*b*) the more accurately described physical stimulus. Most of the data available for answering the question in hand have been obtained by manipulating these determinants. Thus, in the experiment referred to at the beginning of the paper (**22**), one group of lid responses (the reflex) could be obtained with or without the instructions but not consistently without a noise above a given intensity, while another group of responses (voluntary) appeared consistently only when the instructions to wink had been given. As we have already indicated, this grouping of the response attributes according to certain necessary antecedents caused the appearance of definite modes in their distribution, both the average latency and the average opening duration of the lid reaction of the first group (reflex) being shorter than that of the second (voluntary). There

was, however, a considerable amount of overlapping between the distributions. In other words, within the middle limits of the total distribution are found descriptively similar responses with different antecedents, and this is the commonly observed fact that has been responsible for most of the insistence that the categories overlap. If the categories are defined by their antecedents, however, this is an unjustified criticism, for the fact of overlapping response characteristics indicates only that in the region of overlap you cannot read back to the antecedent from the nature of the response, just as you cannot determine from the behavior of a moving physical mass what antecedents were responsible for the initiation of the motion. It would, of course, be convenient if the equation were reversible so that response could be predicted from antecedent and antecedent deduced from response, but the fact that this is not always the case does not remove the possibility that of two identical responses one may legitimately be called reflex and the other voluntary. As James once observed (17, p. 528), "It is the bane of psychology to suppose that when results are similar processes must be the same. Psychologists are too apt to reason as geometers would, if the latter were to say that the diameter of a circle is the same thing as its semi-circumference, because forsooth, they terminate in the same two points."

The second question is, therefore, more decisive: how adequately can response antecedents be differentiated? Are we justified, for example, in distinguishing noise as a condition of reaction from instructions to the subject; or in more general terms, is it at all certain that when the essential aspects of instruction as determinants of behavior are ascertained, they will be discontinuous with so-called physical stimuli and descriptively separable? In view of the appalling lack of exact information about sets and the effect of instructions generally, there is a real possibility that the apparent distinction may rest wholly on ignorance, and that when these phenomena are adequately described, we shall find no line of demarcation between physical stimuli and sets to react, no feature by which we may distinguish one from the other. The question is

answered confidently by the behaviorists who assure us that it is only ignorance of implicit reactions that is responsible for the apparent difference and that ultimately the antecedents of response will all be described in terms of physical stimuli. It is undeniably true that an intricate complex of stimuli (particularly of a kinæsthetic nature) must enter into any set or *Aufgabe*, but it is far from certain that the description will be completed in these terms, for important possibilities remain unexplored such as the presence of central processes, which are not identifiable as stimuli, if that term is defined as physical change impinging on a sense-organ. Whatever the facts may ultimately prove to be, it seems that the issue might well be settled for the time being with a view to clarity and accuracy of statement. Since the set cannot yet be described in terms of physical stimuli, it is misleading to confuse it with these more accurately described antecedents. Boring has put the case aptly in his discussion of 'stimulus' and 'situation.' He says (3, p. 121), "Green light of 505 millimicrons wave length may be a stimulus but my grandmother is not a stimulus; she is a 'situation.' The Aufgabe is a situation. The raison d'être of an *Einstellung* or a determining tendency is a situation. . . . To call these ill defined, effective objects and events stimuli is to pervert a term from its precise meaning." He recognizes the necessity, however, of translating the situation into terms of physical stimuli, as soon as this is possible. The whole question merits more discussion than can be undertaken at this time, but for our present problem it seems evident that a distinction between reflex and voluntary action which rests on the assumption that stimuli and sets are essentially different and may be distinguished in all cases, may prove to be founded on our lack of information. This criterion, therefore, must be regarded as very tentative and subject to experimental verification or denial.

The situation is essentially the same for response correlates of other orders, such as neural level, though there seems to be an impression among some psychologists that they are speaking more exactly if their definitions be stated in neuro-

logical language. It is difficult to see that any advantage accrues from classification on this basis. In the first place, the likelihood of the eventual demonstration of continuity between now discriminable levels in the nervous system parallels, for the most part, the situation just described for stimulus and situation, so that the distinctions in the former terms are neither clearer nor more permanent. Furthermore, from the point of view of a science interested in prediction and control of the behavior of the intact organism, there is much to be said for classifications in terms of determinants which are observable and controllable in such an organism. This is not to imply that correlations between neural facts and behavior are not important. They are, of course, the essential data of neurology and in the complete story of the organism from any point of view they must be included. It seems, however, that much confusion might be avoided if, for psychological purposes, classifications were made on the basis of antecedents and consequents observable in the intact organism; *i.e.*, in terms of situations, stimuli, responses (using this last term broadly to include the data of introspection, and the observable activity of the intact organism).

The other commonly cited correlates of response which have served as distinctions between reflex and voluntary behavior may be passed over with brief comment. The completely unsupportable distinction on the basis of the purposeful and protective nature of the reflex has been refuted many times. Furthermore, it is unnecessary that the more important differences between inherited and learned reactions be discussed as a separate criterion. As far as the concepts are neurological in nature the remarks of the preceding paragraph apply, but where the facts are those relating to the time of appearance of reaction to certain stimuli or situations in the course of the development of the organism, they simply introduce to us the necessity of making provision for the fact that each reaction is a function of more than one determinant of which age of the organism is a very important one.[4]

[4] Skinner (26) has called time and age third variables responsible for changes in the value of the equation $R = fS$.

This partially answers the third question which we have asked above. In the intact organism, even though it were possible to demonstrate permanently unassailable distinctions between antecedents of response, it would be impossible in most instances to find an antecedent condition which affects one group of responses exclusively, for it is increasingly evident that reactions have not one antecedent but many, and conversely that a given antecedent is related to many responses. Thus, though the first short latency lid response, which we have called reflex, requires a physical stimulus of minimum intensity before it appears, and varies with changes in this antecedent, it is also affected by the instructions under which the organism is reacting and other concurrent conditions (**21**). Similarly, though the second response may occur without a noise signal, it is modified in various ways by the presence of, and variations in, this stimulus. These facts are but confirmatory of the current tendency to observe and emphasize the interrelations and interdependencies of all aspects of a given organic situation,[5] and provide a serious objection to a distinction between reflex and voluntary behavior which rests on the identification of one group of responses with any one determinant.

Different Functional Relations between Responses and their Antecedents

In the face of this consistent failure to discover absolute features of response or of response determinants by means of which a reflex or voluntary act may be identified, one other approach to the problem suggests itself. The idea of a

[5] Sherrington (**25**, p. 388) long ago recognized the importance of determining the effect of volitional control on reflexes, and before this Dewey saw the importance of the total reactor even more clearly. More recently Herrick (**14**), Dodge (**7**), Coghill (**5**) and the Gestalt group have made it clear in their various fields that isolated reactions are practically non-existent in the intact organism. Dodge has elaborated the point of view for the specific situation which we are discussing: the distinction between reflex and voluntary behavior. He emphasizes the important fact with which we heartily agree that we are always working with a 'behavior flux' in which a response may begin on a relatively primitive level and be elaborated as it proceeds. Any analysis of this flux into separate responses is recognized as an abstraction, but one which is indispensable to scientific analysis.

functional relation between response and antecedent has long been fundamental in the psychology and physiology of action.[6] In spite of this fact, however, the functional relation itself has never been explicitly set forth as the basis of distinction between reflex and voluntary activity, and although psychology as a science is occupying itself increasingly with the determination of exact relations between variables, it has failed for the most part to incorporate the implications of the method into the principles underlying its categories. In other words, categories remain chiefly descriptive as the data of the science are becoming increasingly functional, and we continue to look for absolute differences in descriptive features of response correlates in order to decide whether a response is reflex or voluntary. The obvious alternative is a classification based on differences in function. In this situation we should ask, then, not whether responses look alike or whether the one is dependent on instructions and the other on a noise of a given intensity, but rather whether there are differences in the nature and amount of relation which responses may show to instructions or to stimulus intensity, or to any other antecedent. As a first result of this procedure, the fact that reactions have common determinants will no longer interfere with their separate classification, provided their relations to the common antecedent are different. In the second place, the questions of distinction between various antecedents become irrelevant to this classification, and even though continuity between physical stimulus and situation is eventually demonstrated, the validity of categories based on functional differences will not be affected thereby.

The outcome of such a method of organization of categories cannot be predicted, nor can the exact limits to categories thus established be set, for the limits are determinable only by empirical findings, and the data which express behavior as a definite function of its antecedents are very incomplete. Certain facts, however, give evidence of the existence of

[6] See Skinner's discussion (26). He says, "In the history of the reflex one positive characteristic has always been given by the facts, the observed correlation of the activity of an effector (*i.e.*, a response) with the observed forces affecting a receptor (*i.e.*, a stimulus)" (p. 438).

functions of widely different orders, one group of which seems to include the reactions commonly called reflex, and another those called voluntary. When, for example, we examine reactions of the eyelid as functions of certain instructions we find (**22**) one group (the voluntaries) in which opening duration is definitely modifiable by instructions to the subject and another group (reflexes) which shows little change in this feature of the response as a result of instruction. Again, it has long been known from the experiments on 'muscular and sensory reactions' that the latency of the voluntary response is markedly dependent on the set or instructions under which the subject reacts. On the other hand, the latency of the reflex shows comparatively little modification thereby, and there is even some evidence for the lid (**21**), that the only apparent effect of set on reflex latency operates in the opposite direction from that on the voluntary, for the former is slightly decreased by relaxation where the latter is increased.

Similarly, it is indicated by certain researches that voluntary latencies tend to decrease arithmetically as the stimulus intensity increases geometrically,[7] while for the reflex the decrease in latency with increased intensity of stimulus is small or absent.[8] To cite but one more example, Allison (**1**) has recently published a study on reflex and voluntary lid responses in which he represents latency of a second response as a function of interval between stimuli, and in so doing finds two completely indifferent functions for reflex and voluntary acts.

Thus far the classification by function seems to result in practically the same division as the previous classification in terms of antecedents, and in this respect the present suggestion constitutes a modification and extension rather than a replacement of the earlier categories. The chief difference is to be

[7] Froeberg (**11**) has reviewed the early work on the subject, pointing out that up to the time of and including his study the relation as stated by Wundt was confirmed by all but two of eight studies. These two studies were used on very few cases. Of his own results he says for vision, "Disregarding irregularities, the time of reaction tends to increase arithmetically as the intensity decreases geometrically." For sound, the increase in reaction time was somewhat more rapid than the corresponding decrease in the logarithm of the stimulus.

[8] Dodge (**8**), Golla and Hettwer (**12**), Tuttle (**27**), Peak (**20**).

found in the fact that when functions are the basis of comparison, a large group of responses, heretofore considered unclassifiable, may be placed logically in one group or the other even though they are descriptively the same and in certain cases have some of the same antecedents. If, for example, two groups of response are identified on the basis of the functional relations of their latencies to stimulus intensity, then the fact that the functions of the group may cross at some point does not bring it about that the responses falling at the intersection are the same or that the categories overlap, for in spite of common antecedents and identical descriptive characteristics they are different in so far as they behave differently in the presence of variation in an antecedent.

Obviously the question arises immediately whether we shall not eventually find continuity between functions as well as in the descriptive attributes of responses and their antecedents, and upon the experimental answer to this question hinges the validity of the suggested basis of classification. The answer cannot be predicted, though it might be expected that such intermediate functions would be found in the relations of the automatic response which from a descriptive point of view has always seemed to fall in between reflex and voluntary action. In our own experience, however, certain of these reactions show functional relations to antecedents which are quite different from the reflex and definitely of the same order as the voluntary. One example from unpublished data on the effect of 'set' on the lid response will illustrate the statement. When subjects instructed to wink voluntarily had been given extensive practice, the secondary winking response became so automatic and habitual that even under conditions of relaxation when the subject reported 'no effort' and complete inattention to stimulus or response, secondary reactions frequently occurred. With relaxation of the subject, however, the latency of the secondary response tended to increase. In other words, even though the reaction had become automatic it continued to behave like a voluntary response, showing marked increase in latency when changes were made in the antecedent conditions of the organism.

Similarly, experiments reported elsewhere (**22**) have indicated that with practice the opening duration of the voluntary lid response may be reduced to values equal to and less than those of the lid-reflex, but it does not necessarily follow from this fact that the voluntary reactions become reflexes, for when the response attribute in question is plotted as a function of repetition under the given experimental setting, it will be seen that two distinctly different functions are obtained. (See **22**, subject Wa, Figure 5.) In the one, opening duration increases with repetition; in the other, it decreases. At the point where the functions cross will again be found descriptively identical responses to the same antecedent, which are, nevertheless, classified separately because of the fact that in the presence of variation in an antecedent they behave differently.

In any event it becomes one of the advantages of classification that as a result of it, important questions of this kind are made apparent. We know a good deal about the descriptive characteristics and the methods of establishing automatic responses, but comparatively little about the manner in which they vary with conditions as compared with other responses. Nor can it yet be stated just where the 'conditioned response' will belong in functional terms, though in purely descriptive terms it is already identified chiefly with the voluntary group (**13**, **15**, **16**, **23**, **29**). Research along these lines is in progress, and the verification of the present classification must await these and other results.

The program of experimentation pertinent to this formulation of reflex and voluntary categories is in a sense inclusive of all behavior research where responses are studied as functions of antecedents. One of the most fruitful approaches is to be found, however, in the experimental situation which was suggested by Dodge in 1926 in an investigation designed to test the drainage hypothesis (**9**). He instructed his subjects to react voluntarily to the same stimulus which excited a reflex and in this setting, responses of different orders were obtained under genuinely comparable conditions. From this point of view alone there are innumerable problems involving

a determination of the various kinds of functions obtainable when response attributes of all kinds are studied in relation to antecedent conditions, such as stimulus quality, intensity, duration, rhythm, repetition of stimulus and of response at various intervals, instructions, 'set,' and many combinations of these and other antecedents. The amount of variability of response about each of these functions will be a further important consideration and may be a secondary ground for a differentiation of response, though it would seem to be a measure of the degree to which antecedent conditions have been controlled rather than a fundamental characteristic of the response.

Such a list of experimental problems bears a suggestive similarity to the situations from which Sherrington (25, p. 14) derived his facts for the classic comparison between the characteristics of conduction in the reflex arc and conduction in the nerve trunk. His distinctions are based largely on a comparison of the different relations which these two processes bear to certain correlates such as stimulus intensity, duration, etc., and it is on just these grounds that we are proposing the distinction between reflex and voluntary response be sought. Incidentally, when such comparative data are available for reflex and voluntary action, it will be of interest to observe whether the direction of difference between their functions may not be the same as the direction of the difference between the functions of conduction in the nerve trunk and conduction in the reflex arc as demonstrated by Sherrington and others. Superficial consideration of results suggests that it might be so. The reflex arc shows less correspondence between intensity of stimulus and extent of response than does the nerve trunk; similarly the amplitude of voluntary response will doubtless show less relation to stimulus intensity than does the reflex. The reflex is more susceptible to inhibition than the nerve trunk; likewise voluntary response may probably be said to be more susceptible to inhibition than the reflex. But this point is both incidental and hypothetical and the present distinctions do not rest upon its verifications. It offers, however, a plausible hypothesis and further justification for the present program.

Wiersma's discussion (**30**) of reflex and voluntary action must be considered briefly at this point, for he has come to a different conclusion from ours on what may seem to be similar grounds. His point is the common one that there is a gradual transition from the reflex to the voluntary act with no obvious line of demarcation, and in support of this he cites the development of the reflex out of the voluntary, the similarity in latent times, the purposefulness of both and more especially the fact, "dass dieselben in mancher Hinsicht den gleichen Gesetzen gehorchen."

Examination of the argument by which he seeks to prove that reflex and voluntary responses obey the same laws fails to confirm this conclusion. His proof of the statement consists essentially in showing that perceptions and other conscious phenomena may be inhibited and facilitated under certain conditions and that, similarly, reflexes may be inhibited and facilitated. He states, for example, that attention tends to increase the *Bewusstseinsgrad* of ideas and sensations and that attention to certain reflexes similarly facilitates them; that simultaneous perceptions inhibit each other (Heymans' law) and that reflexes may also be shown to inhibit each other and to be inhibited by preoccupation with something else. Neglecting the questionable validity of certain of the cited facts, it should be noted, in the first place, that to show that a given antecedent has an effect on two different responses without specification of the nature of the effect or the amount, does not establish the fact that the phenomena obey the same laws. As we have observed, most events in an organism affect the concurrent activity, but the degree and direction of the effect varies widely. In the second place, even though Wiersma had shown that perceptions and reflexes bear the same functional relation to their antecedents, he would not have demonstrated thereby that grounds for the reflex and voluntary categories do not exist, for there is no particular reason, except the parallelistic bias which Wiersma clearly possesses, why perceptions should necessarily be called voluntary acts. Indeed, if it be true that perceptions bear the same relation to an antecedent, such as physical intensity of a

stimulus, as does the reflex act (*i.e.*, the relation stated in Weber's law), this would be good reason for classing the perception with the reflex! On the other hand, it is altogether likely that the report of the perceptual experience, its latency, its intensity, would follow altogether different laws, perhaps those of the voluntary act. If this should prove to be the case, it would not only afford another example of the different functional nature of a voluntary response, but should also demonstrate, incidentally, the significant fact that the verbal report and the experience which is reported are observably different phenomena.

This suggests the evident fact that the possibility of making distinctions in functional terms is not confined to the comparison of reflex and voluntary behavior, though its application to psychological facts has been limited to relatively circumscribed distinctions such as those between pitch and volume, as attributes of sound, and between recall and recognition as kinds of memory. There are numerous other behavior situations, especially certain of those in which common sense distinctions do not seem to be borne out by scientific findings, where such an approach might well prove fruitful. The confusion that has characterized the treatment of the emotions, for example, seems to have depended in great part on the apparent impossibility of distinguishing one emotion from another except where the antecedents to the behavior are known. Cannon's (4) well known observation that the visceral accompaniments of rage and fear are indistinguishable and the Shermans' (24) failure to find differentiæ in the overt behavior of infants both indicate that in the description of the attributes of emotional response, continuity is the rule as it is elsewhere. On the other hand, the fact that knowledge of antecedent conditions helps in distinguishing one emotion from another may not be wholly the result of observational error, for it is altogether possible that it is the nature of the relation between the antecedent and the response which is the differentiating feature, if there is one. It should be added, however, that suggestions of this kind are not meant to imply the superiority of classification in func-

tional terms in all situations. There are, clearly, important distinctions whose chief significance lies in purely qualitative and descriptive differences. Such are the distinctions between tones and colors and perhaps also those between joy and anger. It is where such qualitative distinctions fail that classification in functional terms becomes most useful.

We have not felt it necessary to discuss at length the questions of terminology involved in the use of the terms 'reflex' and 'voluntary.' As we employ them, they obviously do not imply all the different meanings acquired during their long existence. This can and must be made plain in any careful writing. There is continuity with the past, however, which rests principally on the fact that certain identifiable responses have always been and are still classified as reflexes and others as voluntary acts. The implications of the distinctions have changed and the boundaries have shifted, but the old names serve to orient us in the appropriate realm of discourse and this is sufficient justification, where terms are not the data of the science. Physics does not bother to revise the terminology of its general concepts such as magnetism, electricity, or even energy, though these terms have had as shady a past from a scientific point of view as any word in the psychological vocabulary. This preoccupation with terminology is surely a symptom of a somewhat prolonged scientific adolescence which, like most cases of arrested development, interferes with normal functioning.

Summary.—The paper has suggested that responses may be classified in terms of (1) their descriptive characteristics, such as amplitude, duration, locus; (2) their correlates or determinants, such as stimulus intensity, instructions; (3) their functional relations to their determinants. When the categories of reflex and voluntary behavior are delimited by either of the first methods, they are found to be indistinguishable under certain conditions. It seems, however, that the third criterion has certain advantages in the organization of data and in pointing lines of research. Examination of experimentally demonstrated relations of responses to their antecedents suggests that these functions fall into separable

groups; that, for example, two distinct functions are obtained when response latencies are plotted against stimulus intensity, or against instructions. In one group belong those responses which have always been called reflex, and in the other the reactions called voluntary. In addition, other responses, indistinguishable save in their functional relations to antecedent conditions, fall logically into one or the other of these groups. The reflex-voluntary dichotomy, however, may not be permanently defensible even on these grounds, for as new data become available, intermediate functions may be discovered, which cannot be classified under these groupings. In this case the categories will require reconsideration and possible supplementation. As long as classifications are made on the basis of empirically determined functions, however, such necessary changes will in no way affect the structure of the science which is made up of such relations. Indeed, categories may be considered simply as a scaffolding which, indispensable at certain stages of construction, must be modified as the structure grows. It is too soon for psychology to dispense wholly with the convenience and guidance of such concepts, for the relatively small number of established relations in what must be an immensely complex structure, cannot yet hang together without some such formulating principles as these categories afford.

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NOTE ON THE DOCTRINE OF MEMORY-TRACES

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This discussion proposes to examine, in the light of some experimental data, Wheeler's emphatic rejection of the notion of memory-traces (4). Even the use of the term by Köhler himself is unacceptable to Wheeler, for it fails to do justice to the truly dynamic character of the brain-configurations which are the physiological representations of memory. What happens between impression and recall, or, as Wheeler states it, between observation and re-observation? Obviously, something persists, and in response to this crucial and disputed question, it is clear that when Wheeler says that no *traces* persist he does not mean that the original impression has no continuing effect. He simply means that since a memorial pattern is a unified structure in time it is as incorrect, in characterizing the process, to identify parts that may be abstracted from it as 'traces' as it would be to explain time itself by reference to its abstracted parts. Moreover, use of the word 'trace' throws emphasis on the beginning of a temporal pattern, with the assumption that the initial impression determines its entire course, whereas a continuum, be it a straight line, the fall of a rock, the evolution of the species, or a memory-pattern, admits of no such arbitrary distinctions of importance. From the standpoint of the total structure, the beginning is related to the end in no more efficient manner than the end is related to the beginning. This is not to make exception to the accepted time relations between cause and effect, for equal in importance with the temporal priority of the original impression is the fact that except for subsequent development (in which past events are conditional only insofar as they are represented by maturation in the immediate situation) the original impression would be non-existent. Since the trace is so clearly at the mercy of

the laws of growth, Wheeler prefers to avoid the undue weight which the term attaches to the individual character of parts whose identity arises from the analysis. He is concerned, further, with the fact that the term, though useful as a descriptive concept (despite the paradoxical introduction of inflexibility into a problem in dynamics), yet tends to diminish the search for the *conditions* of the event and to obscure the increasingly important fact that energy—whether it be represented by water, light, heart action or awareness—takes its particular configurational nature from the pattern of these conditions. Wheeler thus sees 'traces' not only as superfluous, but as misleading. Whether his protest, in view of the historical development of the problem, is anything more than an insurance against a return to the "evil atomism" of traditional associationism is the question in which we are interested.

The present transition in memory-theory has arisen from an attack upon the two fundamental assumptions of the associationistic account of memory; namely, that the associated terms are, in the first place, *static*, and, in the second place, *specific*. That the first of these assumptions is untenable seems clear from Wulf's demonstration (**5**) that memory-patterns for geometrical forms undergo progressive configurational change. This fact, however, is not entirely disastrous to the associationistic view in terms of particular traces, for it might be argued that these traces, even though developing, retain their palpable and identifiable individual characteristics. But the validity of the second of the above assumptions; viz., the specificity of associated contents, is also becoming more and more doubtful. Having conceded the developmental character of memorial patterns, we may inquire further, whether the operation of these evolving patterns depends on the persistence of *particular* contents, or whether it may occur quite independently of such. If the former, the assumption of traces is indispensable; if the latter, it is gratuitous. In a physical configuration such as water, the presence of the hydrogen or oxygen is not regarded as a *trace:* similarly, if a meaningful configuration conditioned

(or organized) by a given set of words were reproduced without the employment of the original words, the identity or trace-character of the determinants of the reproduction would be of minor, if any, importance, and the term 'trace' itself would be a misnomer.

Adopting Wulf's procedure the writer presented two 100-word legends at an interval of ten minutes to six O's, all graduate students in psychology, with instructions to read through twice with equal attention to all parts of the legends. Reproductions were secured at intervals of 30 seconds, 40 minutes, 24 hours, one week and five weeks after observation, with the instruction to reproduce the legends as nearly verbatim as possible. Among the several results as shown by a qualitative study of the data, the most obvious and expected one, fraught with the greatest systematic consequence, was the relative lack of dependence of both gist and detail upon particular words or sentence-structures. The following is a typical series of reproductions of part of one of the legends, the original passage for which was:

"But the evidence was so equivocal that it was impossible to determine either the fairness of Jones' demands or the exact bearing of the affair to the provisions of the original document."

This was reproduced by observer Go as:

1. Jones' demands, in view of the fact that the situation was equivocal, had to be carefully considered.
2. Because the situation regarding her landing and papers was equivocal, Jones' claims had to be considered carefully.
3. The fact that the situation and the ruling against her was equivocal made it difficult to judge Jones' claims.
4. Jones' claims had to be considered with respect to the somewhat equivocal terms of her exclusion.
5. The ambiguity of the situation made judgment a doubtful and difficult procedure.

For convenience, I have chosen an example where the equivalent contents are all contained in a single sentence; the cases where the homologous members are scattered throughout the legend offer even greater theoretical difficulties to a theory

of specific traces. In the example given there are three main thoughts—(1) Jones' claims, (2) the equivocality of the situation, and (3) the necessity for careful consideration. Though 'haphazardly' arranged, these are all adequately and specifically contained in the first four reproductions and broadly implied in the fifth. Not only is the grammatical structure altered, but the contents are indicated by different words. Thus 'Jones' claims' is equally well rendered 'Jones' demands'; the equivocality of the situation is stated in five different ways with the key word itself, 'equivocal,' changing to 'ambiguity' in the last reproduction. The instances of individual word equivalence were legion, and oftentimes the O did not know that he was changing the wording. Observer Ha rendered the verb 'lay' successively as 'was lying,' 'was reposing,' 'reclined,' and 'was resting,' and thought that he was reproducing the original which, indeed, he was, though not according to the verbatim instruction. A particular word used in the original and appearing in the first one or two reproductions might disappear for a recall or two, being expressed by another word, only to reappear in a later reproduction. Even more striking were the shifts occurring without loss of meaning in cases where several sentences were concerned, involving widespread reorganization.

Thus we find a persistence of organized wholes when there are changes not only in the numerical values of individual parts as in Ehrenfels' 'melody,' or in the exact quantitative and spatial relations of the writer's 'intensities,' (3) but where in addition to alteration in the position of the parts there is a qualitative change of the parts themselves. If we couple the abundant evidence of word-equivalence with the generous transposition even across that natural boundary, the sentence, it would seem necessary to ascribe the appearance of any word or phrase to its relationship to the total dynamic pattern, rather than to the discrete serial running off of specific sensory, imaginal or reflexological mechanisms. The system of traces required by the latter view would be incomprehensibly complex and thus beyond any possibility of verification.

The attempt to get the O's to characterize the nature of

the equivalence proved futile, as existential correlates were conspicuously absent. After the last reproduction, the E presented specific phrases and words which the O had varied, with instruction to choose the one whose occurrence in the original was the most likely, and to report the criteria of decision and the introspective accompaniments. The decisions were uniformly made by the criterion of adequacy in the context which the word or phrase possessed, or its congruity with the story as a whole. The equivalence of a word seemed to consist in its vector or referential character. Thus two words might be used indiscriminately in a given context, either of them referring equally well to some content.

If one discards mechanistic sensationism as an explanation for equivalence, what alternative may be offered? Though the characterization of the 'higher thought processes' in physiological terms is still a somewhat doubtful procedure, there is a constructive suggestion in Lashley's notion of excitation ratios (2). Here the structures, instead of sensory elements of local nerve paths with their stereotyped connections, are "relative amounts of excitation, the spatial arrangements of excited points and stress patterns resulting from the total mass of excitation" (2). While this is not a solution to the problem of equivalence, it provides structures which are sufficiently flexible to make a theory of equivalence possible.

Since the results of the introspective method failed to throw very much light on the nature of the equivalence, the data were examined with the hope of finding uniformities in the meaningful sequences which might correspond roughly with the results of Wulf (5) and Gibson (1) with geometrical forms. The chief questions, as far as the fate of the traces or patterns is concerned, are these: (1) What accounts for the general direction of development? (2) What keeps the pattern together? (3) What determines whether a given detail shall remain or shall drop out in the course of time? With respect to direction of development our data were unfortunately equivocal. We may hope, however, to throw light on the latter two questions by considering in our data

(*a*) the type of detail omitted and (*b*) the disparity with respect to detail omitted between the two legends.

The stories were so constructed that in legend *A*, nine of the twenty-two ideas were logically unessential or irrelevant to the gist of the story, and in legend *B* there were nine such out of twenty-six ideas. The instructions called for uniform attention to all parts of the legend, but in spite of the fact that what would be irrelevant to one O might be relevant to another, it was the same group of details which suffered omission with all the O's. Though in legend *A*, the irrelevant details comprised but forty percent of the total number of details, they accounted for fifty-five percent of the omissions; while the irrelevant details in legend *B* comprised but thirty-four percent of the total, they contributed seventy-one percent of the omissions. In legend *A*, all nine of the irrelevant details were represented in the combined omission column of all of the O's at least four times in the five reproductions, while five of the relevant details escaped any omission. In legend *B*, seven of the nine irrelevancies suffered omission while but two of the seventeen relevant ones met a similar fate. Thus it would seem that memory is directly dependent on relevancy.[1] This conclusion is strongly supported by a consideration of the difference in memory-value between the two legends. Legend *B* was vastly superior to legend *A* in both number of ideas and number of words correctly reproduced. The stories were of the same length and were presented under the same instructions. The order of presentation of the two legends was not an important factor, for the data for observers given legend *B* first accord with those for observers given legend *A* first. Since the conditions were constant, and since the individual factors were in no case heterogeneous enough to effect any exceptions to the superiority of legend *B* over legend *A*, the explanation must be sought in the nature of the legends themselves. Legend *A* was a statement of legal difficulties involved with a certain ship (though there were no technical terms); legend *B* was

[1] The term 'relevancy' is used to refer to membership character of a detail in a larger and established pattern which largely determines its character and to which it refers.

an account of a grade-crossing automobile accident. Roughly speaking, the relations in legend *A* were rational, while those in *B* were chronological or temporal. Now we have just seen that although in both legends relevant details were omitted less frequently than irrelevant ones, the *ratio* of relevant to irrelevant omissions was different in the two cases. In both legends more of the irrelevant details disappeared than did relevant ones. However, this difference in legend *B* was forty-two percent while in legend *A* it was but ten percent. It seems reasonable to infer that in legend *B* the logically relevant details were *psychologically* more relevant than were the logically relevant details of legend *A*. But in this case it is clear that legend *B* should have had a higher memorial-value than legend *A*—which is actually the case.

It would seem, then, that Wheeler is correct in his contention that the recall-recognition phenomenon does not depend on persistence of static impressions but upon something akin to maturation, and that the crucial data of any given memorial event are to be sought in the immediate conditions of the situation under observation. Our data show, in the first place, that a memorial process may go on quite independently of particular contents, and in the second place, that the persistence of those contents that do remain 'stable and identical' is a matter not so much of their nature as of their relevance. Whether or not the term 'trace' should be given up will depend largely upon its interpretation. In the sense of *replica* of the original neural process it is indeed of little value. If, however, it be retained as a descriptive and fictional concept to bridge over an otherwise incomprehensible gap between impression and recall, then it should be regarded in the sense of 'result' of the original neural process.

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DISCUSSION

TERMINOLOGY IN RE "PSYCHOGALVANIC REFLEX"

In a recent helpful article Landis [1] has reviewed the arguments for and against the current and historical terminology used in the field of the galvanic responses of the skin, referring especially to a previous article by Lauer.[2]

In a rapidly growing science like psychology and especially in a field of theoretical and experimental work to which contributions are being made from many different sciences, it is highly desirable to attempt a standardization of terms so that we may all speak the same language and think clearly on the subject. Landis is quite right in distinguishing between terminology used in titles of articles *vs.* terminology consistently pursued within the article. I am also in substantial agreement with his arguments against the use of the term 'psychogalvanic reflex.' Worse than the oft-quoted 'Holy Roman Empire,' which was neither holy nor Roman, but something of an empire, the psychogalvanic reflex is neither psychical, galvanic, nor a reflex. The psychical element is at least a subsidiary and at most only a parallel phenomenon. Even in the case of emotions there is now no more necessity for prefixing this expression than it is necessary to prefix it, for example, to the digestive and salivary reflexes.

'Galvanic' is also an obsolete or at least an obsolescent physical term and in the light of increased knowledge concerning the electrical phenomena of the skin it is no longer directly applicable. Landis's argument against the term 'reflex' is quite apropos. Our usual connotation implies a sensory motor arc. Some sort of sensorium, an afferent nerve, a synaptic center, and an efferent nerve are required in the usual conception of this term. Since the electrical phenomena of the skin appear to be directly related to the sweat glands whose electrical manifestations are conditioned by the autonomic nerve fibers, the term is obviously not applicable.

It will be some time, of course, before we can get rid of the

[1] C. Landis, "Psychogalvanic reflex" nomenclature, PSYCHOL. REV., 1932, 39, 184–188.

[2] A. R. Lauer, Why not re-christen the "psychogalvanic reflex?" PSYCHOL. REV. 1931, 38, 369–374.

historical terminology, especially since a number of writers today have even abbreviated it by using the symbol PGR. We are now in the period of proposing a new term and, since many may be proposed, a selective process is going on. Gradually, however, a precipitation may occur in the solution that now may seem quite cloudy. In the interests of this outcome may I make the following suggestions and proposals:

'Galvanic skin response' proposed by Landis contains two items that, in my mind at least, are objectionable: (1) galvanic in the interests of modern physics might as well or better be electrical; (2) skin is a noun and is only apologetically used as an adjective. My practice is to use nouns in such a relationship only when no adjectives are available or when they are extremely awkward. My proposal, therefore, runs as follows:

Electrical dermal response = (1) Changes in the electrical resistance of the skin to externally applied currents due to a variety of physiological phenomena under the control of the autonomic nervous system following sensory or ideational stimulation usually with affective concomitants [Féré]. (2) Changes in the electrical potential of the skin due to the same conditions as in (1) [Tarchanoff].

In defense of this definition may I point out that 'dermal' is specifically a much used adjective referring to the true skin, which lies beneath the superficial epidermis and contains the cutaneous receptors and also the sweat glands. The *electrical phenomena of the skin* defined separately by Landis would be adequately covered by the above definition. *Psychogalvanic reflex* would be set down as an historical expression covering the phenomena defined in our previous paragraph, the usage of which is no longer recommended.

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